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ART. XXVI.—*Anniversary Address of the President to the Linnean Society, May* 24, 1873.

THE anniversary address this year before the Linnæan Society was devoted to some considerations growing out of "the recent progress of the study of Vegetable Anatomy and Physiology." It is interesting to notice how topics of this sort are handled by a consummate systematic botanist who has never paid attention to vegetable anatomy and physiology, either for their own sake or as a professional expounder, but who appreciates their bearings upon his own department of the science. Mr. Bentham's address opens with the following passages:—

"I have been struck with the observation made by more than one critic in this country, and commented upon in some foreign journals, that we in England are in this respect some way behind our continental neighbors—that, for instance, the most important investigations and consequent discoveries relating to sexual propagation, and the incipient history of cryptogamic plants and microscopic animals, have been made in France and Germany—and that we are, in short, comparatively deficient in what the Germans are pleased specially to distinguish by the name of Scientific Botany and Zoology. Without admitting for a moment that there is less of science in the study of the comparative anatomy, the mutual relations and consequent natural arrangement, and the geographical distribution of the higher animals and plants than in that of microscopic structure, we may acknowledge that there may be some truth in the remark that, with few exceptions, we have not excelled in that long,

patient, and tedious devotion to one subject of limited extent from which such discoveries have usually resulted; and the fact may be, in some measure, the result of our social habits and ideas. Our early education, the whole tendency of our lives, is generally directed to the means of advancement in the world, if not always to the increase of income, at any rate to the raising of our social position in the eyes of those amongst whom we live. If the enormous increase in our commercial and industrial wealth be carefully investigated, it will be found to be in many respects deeply indebted to the recent progress of pure natural science; and yet the necessary study of that pure science will neither enrich the one who would devote himself to it, nor yet raise him in the estimation of his neighbors and associates, whilst it may seriously interfere with his means of bringing up his family, reduced as they become by the rapid increase in the expense of living. We have not in this country those numerous small professorships, or government or municipal places in provincial towns, which give to a man of modest requirements sufficient leisure steadily to carry on his researches year after year without interruption. Content with what he has thus secured, many a continental naturalist looks for no further advancement; he requires no relaxation but perhaps a few weeks in summer spent at a bathing-place; he seeks his reward in the publication of the results of his labors in Transactions or Journals, or a favorable report, without having to calculate on pecuniary results. If we had any such places in this country, few Englishmen could be found to sit down in them to rest and be satisfied; and it has required some moral courage in those of our young men who, having enough to live upon, with a passion for science, have for its sake renounced all attempts to climb round after round on the social ladder. We have had, however, and still have such men. With all our social drawbacks we have contributed our fair share to the progress of natural as well as of physical, mathematical, and other sciences. We have had our Robert Brown, and long before him our John Ray. Among our living zoologists and comparative anatomists I could name those who yield nothing to any of their continental rivals; and above all we must remember that it is an Englishman who has, in this nineteenth century, brought about as great a revolution in the philosophic study of organic nature, as that which was effected in the previous century by the immortal Swede. With such names as Linnæus and Darwin, the northern nations can well hold their own in the presence of any scientific celebrities of Central Europe."

"In considering these observations it may not be uninteresting to keep in mind a perceptible difference between our two great scientific neighbors, the French and the Germans. Excelling in

method, the French are unrivalled in clearness of exposition in Natural History, as in Mathematics, Jurisprudence, Philosophy, and other abstruse subjects. With a great readiness to seize the general bearings of the several facts or points they have before them, they will at once organize them into systems or theories, often successfully; but they may be sometimes apt too readily to admit into these systems and theories elements which they have not verified, or not to wait for a sufficient confirmation by repeated observations of the original facts upon which they were founded. On the other hand, method and exposition are not among the distinguishing characters of German naturalists; they have had no Jussieu, no De Candolle, no Cuvier, nor, in earlier days, had they a Tournefort or a Buffon; but they are beyond all competition in laborious and patient investigation of details upon which all reliable conclusions must be founded; to them also we practically owe the greater number of important compilations, Genera and Species, Nomenclature and indexes, Records, &c., equally requiring steady labor, with results not brilliant, but useful. Again, if the French are good theorists, the Germans are great speculators. If French theories may sometimes be found defective in detail, so German imagination is apt to wander too far from the facts from which it started. And this comparison of French method and German detail, of French theory and German speculation, will probably be found exemplified not only in their physiological researches and elementary works, but also in their monographs and other systematic publications. You learn more rapidly from a Frenchman; the German supplies you with more materials for study; and thus you derive equal benefit from both."

The most important portion of the address is that which relates to

The questions of Gymnospermy and Genealogy.—"An important question in vegetable morphology, first brought forward by Robert Brown, and a subject of much controversy in later times, the gymnospermy of Conifers and their allies, has recently been placed in a somewhat new light by a German physiologist. The nucleus and, later, the seed proper (that is, the embryo and its albumen) are in these plants enclosed in fewer envelopes than in any other phænogams. Many Monochlamyds or Monocotyledons have no perianth or stamens round their female organs; but in all, except these Gymnosperms, the nucleus or embryo is enclosed in a simple or double integument within, but distinct or distinguishable from, a carpellary envelope. In Conifers and their allies the simple or double integument alone covers the nucleus. R. Brown, after a long series of careful observations, published, in 1825, his conclusions that this simple or double integument corresponded to that of the

ovule and seed in other Dicotyledons, and that Conifers have no ovary, style, or stigma.* Lindley observed, in 1845 (and left the observation unaltered in 1853), that 'about the accuracy of this view there is at this time no difference of opinion.' Since then, however, Payer, and his disciple Baillon, founding their conclusions upon organogenesis, have asserted that it is the seed-integument, not the carpellary envelope, that is deficient—a view which has been supported by Parlatore and others, refuted by Hooker, Caspary, Eichler, and others, and again taken up by Prof. Strasburger, of Jena, after a series of careful and detailed organogenetic observations, combined with genealogical, or, as they term it, phylogenetical considerations, in a remarkable essay entitled 'Die Coniferen und die Gnetaceen.' In the attempt to reconcile views apparently so opposite, taken by naturalists whom we should all consider of high authority, we must, perhaps, in some degree, take also into account a certain bias which may be observable on either side. From the well-known accuracy of Brown's observations and the soundness of his views in every department of botanical science he entered into, there is a great disposition on the one side to rely absolutely on his conclusions; whilst on the other hand, French organogenesists, having broached theories which have proved of great importance in various homological questions, have been but too ready to set them up against all authority, without sufficient verification of detail. In the present case this verification of detail has been supplied by Strasburger, who has combined it with general considerations now first brought to bear on the gymnospermy of Conifers. He proves to be an ardent disciple of Häckel, the greatest amongst Germanizers of Darwinism. The testimony in favor of the derivative origin of forms and organs has certainly received large accessions from the German accuracy and copious details of Häckel and his followers, but at the same time has been the occasion of a free display of German imagination, as I hope presently to show, in considering Strasburger's views of the homologies of Conifers, in conjunction with some parts of Häckel's last great work, the Monograph of Calcisponges.

"In the first place, we must be careful to consider what we mean by homologies of organs. They are of two kinds:—(1) the homology of the several appendages to the axis of one and the same plant, which in zoology may be compared to the homology of the front and hind limbs or of the several vertebræ

* Strasburger, in an historical sketch of the progress of the question, points out that Targioni-Tozzetti in 1810 enunciated views very similar to those afterwards developed by Brown. Published, however, in a journal which had but very little circulation, his notes remained almost unknown till attention was called to them by Caruel in 1865. Strasburger quotes the passage (with some typographical errors), p. 174 of his "Coniferen."

of one and the same animal; and (2) the homology of the organs of two different plants, corresponding to the homology, for instance, of the wing of a bird with the fore-leg of a quadruped. To the former class belong the various much-vexed questions on the distinction between axis and appendages, arising in the consideration of the flowers of Conifers as of many other orders; but it is the latter class with which we are now more specially concerned in relation to Brown's gymnospermous theory. In his time this homology of organs was determined solely by their similarity in position, development, structure, and other characters, as observed in the plants compared: in the present day physiologists have to take into account the evidences, either of their hereditary derivation from a corresponding organ in a common parent, or of their being an early stage of development of organs which have further progressed in plants to which their own race are supposed to have given birth. It is in this respect chiefly that the arguments put forth by Strasburger differ from those of his predecessors. But whilst giving him every credit for his patient and persevering elaboration of details, we cannot but see in his derivative arguments much of purely imaginary mixed up with well-attested evidences. When in the higher races of phænogamous plants we meet with staminodia, carpidia, or other rudimentary or anomalous productions, we may justly, with Darwin, conclude that they are the hereditary representatives of organs normally perfect in some parent race, but which, in consequence of other adaptations of the general economy of the plant, have, in the course of successive generations, become useless and gradually reduced or almost obliterated, if not modified so as to perform different functions. So when we find in a species, or group of species, some one organ specially modified in adaptation to special purposes, and thus differing or progressing from the forms prevalent in the genus or order to which it belongs, without retrogression in other respects, and if we allow no fallacy to creep in as to what we mean by progress or retrogression, we may perhaps conclude that we have at the same time a specially modified race and unmodified descendants of the race it has sprung from. But it is hard to believe that Strasburger had any such solid foundations for his argument that the envelope of the nucleus of Conifers is genetically the same as the carpellary envelope of the higher Phænogams. He does not, as far as I can learn, pretend that this envelope is the reduced representative of organs more perfect in previous races; for the presumed ancestors of Conifers are cryptogamic. He rests solely upon the supposition that this envelope in Conifers is the first appearance of an organ further developed in the outer integument of their descendants, the Gnetaceæ, and perfected in the carpels of their ultimate progeny,

the higher Dicotyledons. But there seems to be very little beyond pure imagination upon which to found such a supposed pedigree; and many reasons present themselves against the belief that the higher Dicotyledons can have descended from Gnetacæ or Gnetaceæ from Conifers, or that Conifers ever produced any races now existing out of their own order. As a postulate under the Darwinian theory, we may allow all to have had their origin in a common parent. We may also, from the scanty evidences supplied by Tertiary and Cretaceous remains, believe that the parent races of some of our species, or perhaps genera, may have remained unchanged to the present day in company with their modified offspring. Even of two nearly allied orders one may be more altered from the common stock than the other, and may be thus in a vague sense said to be derived from it and therefore more modern. Thus Cycadeæ may be supposed to be more ancient than Conifers, Araucariæ more ancient than other groups of Conifers; but the common parent of Conifers, Gnetaceæ, and other low Dycotyledons belong to an age so remote as to have left no visible trace to guide us in our conjectures.

"From such conjectures, however, as have been indulged in by phylogenesists, I gather that the supposed earliest progenitor of the plant-races was a simple organism multiplying by internal growth and division, that at a later stage, besides growth in various directions with a tendency to radiation, sexual elements had arisen, at first, perhaps, without other arrangement than their proximity. From that stage the progress toward the more perfect plant became multifarious, some of the principal courses followed being the differentiation of the indefinitely growing axis and its definite appendages—the respective arrangement of the male and female element, of the female at the end of an axis or of one of its branches, and of the male on the appendages—the adaptation of the appendages to the various purposes of vegetation, of protection to the sexual elements, or of assisting them in their functions—the separation of the male from the female element, &c. I see no arguments to oppose to these different modes of gradual progress by means of natural selection through a long succession of untold generations; but they cannot have followed the same sequence in all races of plants. In some the separation of sexes may have long preceded the development of floral envelopes; in most of the higher Phænogams the reverse has been the case. Phyllotaxy has become highly developed in several Cryptogams, while in some Phænogams, far advanced as to sexual apparatus, the foliar system has remained in arrear. But in none of these courses have we any evidence of retrogression. We have no more reason to believe that sexes once separated are brought together again in future generations than that cellular plants should descend from

those in which the vascular system has been perfected.* And yet we must believe this if we admit Strasburger's pedigrees. We must suppose that races, after having once secured the advantages of a total separation of the two sexes and undergone modifications suited to their separate requirements, have again returned to their primitive state of sexual proximity, and commenced a totally different series of modifications destined to counteract the evil effects of that proximity. A much more simple hypothesis would be that Conifers separated from the common stock before the development of floral envelopes, the higher Dicotyledons before the separation of the sexes. The arrangement of the vegetative organs, or phyllotaxy, had probably acquired considerable perfection before the separation of either of these primary classes of Dicotyledons; for we have the verticillate arrangement in alternating whorls in *Frenela*, *Ephreda*, *Casuarina*, *Calycopeplus*, *Hippuris*, and many others belonging to the most widely separated natural orders—the opposite and decussate leaves in various genera of Conifers and Gnetaceæ, as well as in numerous orders, whether of Monochlamydeæ, Gamopetalæ, or Polypetalæ; and in Conifers, as in the higher Dicotyledons, the whorled or decussate arrangement is variously broken up into the spiral, the alternate, or the scattered. But the reproductive organs having at that early stage taken the two directions of total separation of the sexes in the one and their union in the other within a set of floral envelopes, their progress was thenceforth in different directions, and homology in a great measure disappeared. In Coniferæ this complete separation of the sexes and fertilization through the agency of wind being established, natural selection would only promote the development of such floral envelopes as might be required for protection and would not interfere with the fertilizing process, and would necessarily be very different in the male and in the female flowers. Accordingly one great point established by Stasburger and others is, that in Coniferæ and Gnetaceæ there is no homology between the male and the female flowers. In the higher Dicotyledons the male elements took their place around the females, and axial appendages would be early established or modified for the various purposes of assisting, protecting, or controlling fertilization or maturation, all of which arrangements would become more and more complicated as the plants came to be benefited by cross fertilization through insect and other external agencies, or again simplified by partial abortions

* The apparently exceptional case of unisexual flowers, supposed to have descended from perfect hermaphrodite ones by the gradual abortion of one of the sexual elements, in which the abortive element is occasionally again perfected, is no real retrogression. An occasional perfect stamen in a female Euphorbiaceous flower cannot be said to be a real return to hermaphroditism.

as the same purposes came to be answered by more or less perfect unisexuality or other means.

"If, then, we are right in concluding that Gnetaceæ cannot have descended from Conifers nor the higher Dicotyledons from Gnetaceæ, though all may have descended from a common stock, we cannot but think that Strasburger has failed in proving any genetic homology in their floral envelopes. The question returns, therefore, to its old phase, to be determined by morphology, position, and functions.

"First, as to morphology. In phænogamous plants, immediately around or among the sexual elements the outgrowths from the floral axis are of two kinds, either continuous and uniform or oblique all round the axis, or arising in several separate parts: the former are regarded sometimes as mere axial developments, sometimes as exceptionally single and one-sided foliar organs; the latter as appendages or leaf-organs, forming part of the general phyllotaxy of the plant. To the former class would be referred discal excrescences and ovular integuments, to the latter carpellary elements. Strasburger shows that the disputed envelope in Conifers most frequently, though not always, appears at an early stage in the shape of two more or less distinct opposite protuberances, that it is consequently foliar, partaking of the phyllotaxial system of the plant, not axial nor exceptionally monophyllous and unilateral, and that it is therefore carpellary, not ovular.

"But here we have another element of uncertainty, which has recently been the subject of much controversy, and to which I shall presently revert. The limits between axial dilatations and regularly formed appendages are not always definite, and occasionally are wholly obliterated; and the present case may be included amongst those in which the distinction is ambiguous. Morphologically the seminal envelope of Conifers shows a tendency to enter into the general phyllotaxial system of the plant; but in several genera it retains the characters of an axial dilatation, or, as Strasburger interprets it, a single leaf. In *Gnetum* there is a double inner integument, which he considers entirely ovular or seminal and monophyllous, whilst the outer one is, according to his view, carpellary, consisting of two leaf-organs in conformity with the general phyllotaxy; but he admits (p. 119) that the outer one of the two ovular integuments is traversed by bundles of vessels similar to those of the external carpellary envelope, and "only affords a further proof of the morphological connection of the two.

"In position, the integument of the Coniferous nucleus appears to me to be similar to that of the ovular envelope of the higher Dicotyledons, close around and on the axis terminated by the nucleus, not that of the carpellary leaves, which are on

a different axis. Whatever be the theoretical origin of the ovule of the higher Dicotyledons, on the margin or in the axil of the carpellary leaf, or on a prolongation of the central axis, its funicle, which bears the integument as well as the nucleus, is a branch, and therefore a secondary axis, and not the main axis of the flower, on which are placed the carpellary leaves.

"In function, the integument in question is purely ovular and seminal, the protection of the nucleus and embryo, not that of the carpellary leaves of the higher Dicotyledons, which bear each a separate stigmatic apparatus for the reception and transmission of the pollen-tubes to the nucleus. This, however, is a purely adaptive character, whose chief value is in respect of practical terminology.

"The result of the above considerations as to the homology of the integument of the nucleus of Conifers as compared with those of the higher Dicotyledons, if I have put them fairly, would therefore be, that genetic homology does not exist, morphological homology is vague and doubtful, position indicates rather that of the ovular or seminal than of the carpellary integuments, so also does the secondary and adaptive homology of function. Theoretically, therefore, we should say that the organ in question is not the exact homological representation of either the carpellary or the seminal integument; but practically it is most useful and instructive to treat it as seminal. And as to the name of the two great subclasses of Dicotyledons, as all are agreed that they are essentially distinct, in that the one is deprived of one of the two envelopes (carpellary and seminal) which exist in the other, the received names Gymnosperms and Angiosperms appear to be really appropriate, as denoting a fact admitted by both sides, though differently interpreted; whilst the proposed names Archisperms and Metasperms are founded on a theory which, under the above views, we cannot but qualify as purely imaginary.

"A valuable portion of Strasburger's essay consists in his detailed illustration of the development of the flowers of *Welwitschia*, an important contribution to the completion of that history of this plant so thoroughly worked out by Dr. Hooker, so far as the materials at his disposal admitted, in his now celebrated paper in the twenty-fourth volume of our Transactions. Hooker had then no flower-buds at his command; and it was only some years later that he succeeded in procuring from Mr. Monteiro more satisfactory specimens, in various stages of development. The various works he was then engaged in prevented his resuming the subject himself; but he transmitted a series of these specimens to Professor de Bary; and it was from these materials that Strasburger was enabled to trace the progress of the flowers from the earliest stage. After an evidently most care-

ful examination, he has given the results, pp. 91 and 141 of his 'Coniferen und Gnetaceen.' The accuracy of his observations has been confirmed by Professor M'Nab, to whom Dr. Hooker had also communicated some of Monteiro's specimens, and who, after an equally careful independent examination, embodied the results in a paper read at our meeting of the 16th December last and now in the printer's hands, to which he afterward added a note on the receipt of Strasburger's essay.

"The chief interest attached to this extraordinary plant lies in the probability of its being the nearest approach to (the least modified amongst the descendants of) the original type or parent stock of Dicotyledons wnich has reached recent geological periods. If, as above, we suppose the original parent race of Dicotyledons to have been one in which phyllotaxy had already become variously modified for the purposes of nutrition, but in which the sexual arrangements remained much in arrear, we may conjecture that amongst its immediate descendants there was a tendency to vary both in the relative arrangements of the sexual elements and in the development of floral appendages amongst and around them, combinations arising in both directions calculated to promote the welfare of the race. In the midst of the varied circumstances in which their descendants were placed in the course of their dispersion through successive ages, some profited by an increasing complexity in their floral developments counteracting the evils of sexual contiguity, others by a total separation of the sexual elements rendering their comparative exposure rather beneficial than prejudicial. From the former may have descended the higher Dicotyledons, from the latter the Conifers;—the former ever increasing in the complexity of their arrangements, so long as they retained their hermaphroditism, simplifying them again, perhaps, in some cases by arrest or obliteration as they become more or less unisexual; the latter retaining rather more of their primitive simplicity. *Welwitschia* does not absolutely belong to either, and may be a race which has come down to us with less of alteration from the early descendants of the common stock than either of the others. Some progress had been made in both directions. Sexual separation predominated, but not until some floral development had taken place; and neither had been carried to the perfection exemplified in the two great subclasses; and the race would probably have become long since extinct had it not been established in a country which has apparently experienced since very early times less of the vicissitudes affecting organic life than any other, and had it not been at the same time endowed with other constitutional peculiarities, enabling it better than any other plant to bear with the physical conditions surrounding it.

"All this may be rejected as purely conjectural; but surely Strasburger's genealogical tree is equally so. My object is merely to show that the supposition that, of the three races now so distinct, *Welwitschia*, after the first variations, has remained the least modified from the common stock, that the Conifers have undergone a greater progressive change in one direction, and the higher Dicotyledons a still greater advance in another direction, is more plausible than the assertion that Conifers are the parent race from which Gnetaceæ have directly descended, and that these, again, have engendered the higher Dicotyledons.

"The establishment of direct pedigrees or genealogical trees, in which the parent and descendant races are supposed to coexist in the present day, is a favorite speculation of the German school, especially since, after Häckel, it has adopted Darwinian views, carried in many instances far beyond what is warranted by the works of the great master himself. In plants at least, such pedigrees appear to be wholly admissible, so long as we have no geological record to justify them. If the image of a tree be really applied to the illustration of the parentage of plant-races, it must be very differently conceived. Taking, for instance, the Dicotyledonous class, we might suppose a tree, in which the trunk represents the common ancestor, forming in successive generations innumerable more or less diverging branches, the greater part of which perish either immediately or in the course of few or many generations, but some remain as branches or common trunks for future ramifications. We may suppose the center of the tree always to consist of those which retain most of the ancestral characters, the lateral branches diverging more and more as they have become more and more modified. These modifications, even the extreme ones, may be for a long time very slight; but in the course of ages (as we may observe in varieties of modern species) some of them may have acquired a more marked character as well as more or less of fixity. We may suppose this to be going on through millions of ages, innumerable branches, whether near the center or more or less distant from it, ceasing to grow or to branch out, leaving gaps in the upper part of the tree, partially filled up, perhaps, in a few instances by returning branches from the circumferential ones, and all decaying at the base, leaving only their upper extremities to continue the process in future ages. We should then have the present races represented by the countless branchlets forming the flat-topped summit of the Dicotyledonous tree—a hundred to a hundred and fifty thousand, perhaps, if we take into account species only, ten times as many if we go into subspecies and varieties; the branches which immediately bore these present branchlets, as well as the lower more general ramifications, will have wholly disappeared from our view, or left only here and there the most

fragmentary traces; and the surviving branchlets themselves will be most irregularly placed. Here we should see thousands crowded into compact patches definitely circumscribed at every point (Compositæ, Orchideæ, Gramineæ, &c.); there we should meet with enormous gaps, either quite unoccupied or a few solitary branchlets or small clusters isolated in the middle (*Moringa*, *Aristolochia*, *Nepenthes*, &c.). In other parts, again, irregular masses may be more or less connected by loosely scattered branchlets or clusters, obliterating all boundaries we might be disposed to assign to them (many of the bicarpellary gamopetalous orders, the several curvembryous orders, &c.). In the imaginary construction of such a tree, all we can do is to map out the summit as it were from a bird's-eye view, and under each cluster, or cluster of clusters, to place as the common trunk an imaginary type of a genus, order, or class, according to the depth to which we go. If we believe that this type, or original trunk branch, is exactly represented by (has descended unchanged to) one of the present branchlets, we place it immediately under that branchlet, as having been directly continuous with it, and regard the remainder of the cluster as the persistent summits of lateral offsets. If we consider that the direct trunk-race of a cluster has become extinct in its precise form, and has left descendents only from its branches, we place it under one of the gaps in the cluster or under a vacancy outside the cluster, according to the conjectures we may think the most plausible, as derived from the relative structures, geographical relations, &c., of the present branchlets or other evidences we can bring to bear upon the question. Such circumstantial evidence will always be exceedingly vague and inconclusive; and the assistance we can derive from the geological record is so exceedingly slight, especially if we descend below those tertiary times in which the ramification was not very materially different from that now exhibited, that in the construction of our tree much must be left to the imagination. Still, as real affinities and geographical relations come to be more carefully studied, and as here and there missing links are discovered, either among geological remains or still lingering in some unexplored region of the globe, we may yet hope gradually to obtain a fair outline of the lost ramifications of our dicotyledonous tree, provided we are always on our guard against the common error of treating plausible conjectures as established facts."

The distinction between appendicular and axial organs, i. e., *between leaves and stems*.—"There is no doctrine better established, no one which has been found practically useful in the history of the life and relations of plant-races as well as of individuals, than that of the homology of appendicular organs as distinguished from the axis—a doctrine originally sketched out by

Linnæus,* poetically conceived by Gœthe, and philosophically worked out by several of the most eminent botanists. Upon this depends the whole system of phyllotaxy; and many an important question of affinity must be decided by a due discrimination of appendicular and directly axial organs or parts. There are, however, cases where such a precise determination has proved difficult or impossible. The leaves of *Pinus*, the outer casing of inferior ovaries, the floral cup of *Myrtaceæ*, some parts of Coniferous flowers above alluded to, the stamens of *Euphorbia*, &c., have led to much controversy as to whether they are axial or appendicular. Amongst other arguments it has been endeavored to decide the question by tracing the development and course of the vessels. It has been found, however, that the main principles of growth and arrangement of the vessels are the same in both, and that in fact no positive line of demarcation in this respect can be drawn between an axial development and a true appendage. It is consequently argued that there is no real difference between a leaf-organ (or appendage) and a branch; and Trècul (Comptes Rendus, 1872, lxxv, 655) goes so far as to propose the suppression of the former term, and call all the parts of a plant branches. To ignore in Nature all classification where no positive limits can be assigned, would be to abolish all method in its study. If we treat all the parts of a plant as physiologically the same, and only give them distinct names according to their functions, we put an end to all study of homologies and affinities, excepting such as are based on the very secondary adaptive characters. If a leaf or a part of a leaf is capable of being occasionally converted into an axis, if the end of an axis may occasionally develop into a definite leaf, if there are a few cases in which the exact point where the swelling of the axis terminates and the leaf-organ commences cannot be fixed, if the differentiation of the axis and its appendages is in many Cryptogams imperfect or null, these are not reasons sufficient for ignoring the real, almost constant, and important differences exhibited by the two classes in phænogamous plants generally.

"At the same time, the demonstration of the susceptibility of ramification of the leaf-organ, which we chiefly owe to French naturalists, is a great point gained. If it takes place in a true vegetative leaf, it results in its conversion into a true bud-bearing axis; if in the floral organs, they may still retain the determinate appendicular character."

We should hardly have regarded Gœthe's morphology as "poetically conceived." It seems to us to have been as soberly, if not as "philosophically worked out" by him as "by several

* See "Prolepsis Plantarum," in the Amœnitates Academicæ, ed. Schreb. vi, 324, where Linnæus shows by a number of examples the homology of bud-scales, leaves, bracts, calyxes, petals, stamens, and pistils.

of the most eminent botanists," due allowance being made for the time. While criticizing Trécul, we are not clear that Mr. Bentham has sufficiently considered the writings of Van Tieghem, who claims, perhaps too confidently, that he can draw a positive line of distinction between an axial development and a true appendage.

"*Hermann Mueller's 'Befruchtung der Blumen durch Insecten'* proves to be just such a repertory and digest of recorded facts supported by original observations as is become absolutely indispensable for the further pursuit of inquiry in this direction. The author is brother to Fritz Mueller, of Desterro in South Brazil, so well known as a judicious and reliable observer, and as a warm supporter of Darwinian theories; and Hermann Mueller himself proves to be an equally persevering and indefatigable collector of facts, having for the present purpose the great advantage of being evidently as well versed in entomology as in botany. It appears also that he has been already assisted by his son Hermann. As far as a hasty glance over the work enables me to judge, the principal general facts here first brought prominently into notice appear to be, the variety of insects which visit the same flowers, the variety of flowers visited by the same insects, and the number of flowers which an insect, deceived by false appearances, visits in search of what is not to be found,—all much greater than had hitherto been supposed.

Besides the methodical record of all the facts he has been able to collect from German, Italian, Swedish, and British literature, H. Mueller commences with a short historical introduction, in which he does full justice to his predecessors, and concludes with some general considerations of a remarkably sober character. He justly criticizes the fanciful flights of Delpino's imagination, to which I have myself alluded in former addresses, and Axell's theory that the development of the fertilizing arrangements in Phanerogams has been always an advance, and still continues to advance, in one and the same direction toward perfection; and, as far as I can see, his own conclusions are none but what are fairly deducible from the facts he records.

"With this book in hand, I cannot but strongly recommend the further pursuit of an inquiry, still in a very early stage, to all naturalists residing in the country, and especially to those who may be located in regions which, like the Mediterranean, the South African, the Southwest Australian, the subtropical and extra-tropical South American, and the Mexican, appear to maintain at once a great variety of locally restricted endemic plant-races, and a great number and variety of flower-seeking insects,—in order that we may ascertain how far these two great supposed facts are confirmed by direct observation, and how far they may mutually have influenced each other."

ART. XXVII.—*Chemical Papers from the Massachusetts Institute of Technology.*—No. II. *The determination of Lead as Peroxide;* by W. C. MAY.

IN the account of Luckow's scheme of analysis of copper ores by means of the galvanic battery, which is given by the officers of the Mansfield Mines,* the behavior of many metals toward the electric current is noticed, and the statement is made that lead is completely precipitated as peroxide on the positive electrode. These authors appear to have considered chiefly the case of a solution previously treated with sulphuric acid, which, consequently, could contain only traces of lead, and I have been unable to find any published account of determinations of lead by this process. The experience in this laboratory confirms the statement of the German authorities; and it has been the custom here, for two years past, in the separation of lead from copper, to precipitate by the battery the small amount of lead which remains in solution after the main portion has been precipitated by sulphuric acid. In order to discover whether this method is serviceable for the estimation of larger quantities of lead, the following experiments were made.

The substance operated upon was brass, the per cent of copper and lead in which were accurately known from a large number of analyses made in this laboratory. Various amounts of proof lead were added in order to increase the quantity of that metal. These were dissolved in nitric acid and diluted with water, so that for one gram of copper about 80 to 100 c.c. of water and 3 c.c. of strong nitric acid were present, in addition to the acid combined with the metals. The metals were precipitated by the galvanic current, the copper upon the negative and the lead upon the positive electrode, using for the former a piece of platinum foil about 2½ to 2 inches; and for the latter a narrow strip of foil, about ⅛ inch wide and long enough to reach to the bottom of the beaker. After the precipitation was complete, the copper was thoroughly washed and was then weighed in the usual manner. The oxide of lead precipitate, I satisfied myself, was virtually insoluble in nitric acid of the strength here used, and therefore deemed it unnecessary to continue the current after the copper had been removed from the solution. All chance of error in this respect might be avoided by connecting a platinum wire with the battery, in place of the negative electrode, and immersing it in the solution. After drying and weighing the copper, the lead

* Fresenius Zeitschrift, 1869, p. 1.

which remained upon the positive electrode was gently chafed off, with a rubber-coated glass rod, into a small beaker containing water, and the entire precipitate was collected upon a filter, washed and dried, and then transferred to an accurately weighed porcelain crucible and ignited gently, apart from the filter. This treatment brought the lead to the state of protoxide, in which condition it was weighed. From this weight of protoxide the per cent of lead was calculated. I generally found no trouble in completely removing the peroxide of lead from the foil; should any difficulty be experienced, however, the last traces may be converted to the peroxide upon the platinum itself, by cautious heating; and without, in the least, injuring the foil.

Below are the results of several analyses.

I.

Wt. of copper taken = 2·3723	Wt. grms. of lead taken = 1·7804 grms.	Total 4·1527
" " found = 2·379	" " " found = 1·7791 "	
Per cent " " = 57·28	Per cent " " = 42·84	
Theory = 57·13	Theory = 42·87	

II.

Wt. of copper taken = 0.3988 grms.	Wt. of lead taken = ·3857 grms.	Total ·7845
" " found = ·4001 "	" " found = ... "	
Per cent " " = 51·00 "	Per cent " " = ...	
Theory = 50·84	Theory = 49·16	

III.

Wt. of copper taken = ·4740 grms.	Wt. of lead taken = ·0907 grms.	Total ·5647
" " found = ·4737 "	" " found = ·0895 "	
Per cent " " = 83·88	Per cent " " = 15·84	
Theory = 83·94	Theory = 16.06	

IV.

Wt. of copper taken = .5461 grms.	Wt. of lead taken = ·0246 grms.	Total ·5707
" " found = ·5458 "	" " found = ·0240 "	
Per cent " " = 95·63	Per cent " " = 4·20	
Theory = 95·69	Theory = 4·31	

V.

Wt. of copper taken = ·2201 grms.	Wt. of lead taken = ·0260 grms.	Total ·2461
" " found = ·2205 "	" " found = ·0248 "	
Per cent " " = 89·59	Per cent " " = 10·07	
Theory = 89·44	Theory = 10·56	

VI.

Wt. of copper taken = ·4666 grms.	Wt. of lead taken = ·4109 grms.	Total ·8775
" " found = ·4695 "	" " found = ·4070 "	
Per cent " " = 53.50	Per cent " " = 46·38	
Theory = 53·17	Theory = 46·82	

These results were obtained by the process previously described and under the conditions there detailed, and seem quite satisfactory, especially the first three. The plus error of the copper was probably due to slight oxidation of the metal upon the edges.

In a subsequent analysis I added to the copper and lead solution a solution of iron and potash, which, so far as can be judged by a single determination, exerted an injurious effect upon the precipitation. Further investigation must be made to ascertain this more fully. Following is the result of the analysis in this case.

VII.

Wt. of copper taken = ·6153 grms.			Wt. of lead taken = ·3252 grms.		Total ·9405
" " found = ·6126 "			" " found = ·3027 "		
Per cent " " = 65·13			Per cent " " = 32·18		
Theory = 65·42			Theory = 34·58		

ART. XXVIII.—*On the Quartzite, Limestone and associated rocks of the vicinity of Great Barrington, Berkshire Co., Mass.;* by JAMES D. DANA.

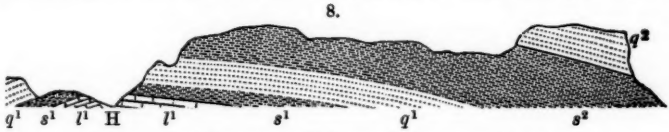
[Continued from page 91, vol. v.]*

3. *From the Housatonic Valley eastward.*

THE region about to be described is within the same half a dozen square miles of area that have afforded the facts mentioned in the preceding parts of this memoir. It lies to the south and southeast of Monument Mountain, and is within three miles of it. And yet it is independent in the flexures of its rocks, and exhibits alternations of quartzite, mica schist or gneiss, and limestone beyond what has been elsewhere observed.

It will be remembered that Monument Mountain† crosses the north and south Housatonic region obliquely from northwest to southeast, and that the section along the course of the mountain included on the west a low anticlinal of limestone (l) beneath mica schist (s^1), and then, eastward, in the mountain, a succession of strata of quartzite, (q^1, q^2) and gneiss (s^2, in part

8.



Section across Housatonic Valley and Monument Mountain.

mica schist), dipping 20° to 25° to the southeastward, as represented in the annexed section (fig. 8). At the east end there is no change to a westward dip, so as to make the whole a gentle

* The preceding parts of this Memoir are in vol. iv of this Journal, pp. 362, 450, and vol. v, pp. 47, 84.

† See this Journal, III, iv, 450.

synclinal; instead, the slope continues on and terminates in a fault, as explained beyond.*

While this is the position of the rocks in this Monument Mountain section, there are, but a mile north of the position of the mountain—as shown in the Glendale section—two steep and small synclinals (V^1, V^2, fig. 9) of the same schist and

9.

Section from Glendale westward.

limestone, with intervening anticlinals, the limestone being the prevailing and underlying rock, between whose jaws the schist was pinched up; and, further, these two small synclinals narrow out within a mile to the south against Monument Mt.†—each trough where widest being too narrow to take in much of the overlying schist, and including less and less as it extends southward.

Now in the section through Great Barrington,—three miles south of the east end of Monument Mountain and four miles of the west end (and separated from it by open meadow land)—the position of the rocks is still different; and the rocks themselves, although equivalents, are also to some extent different. There is a fault, as in the Monument Mountain section; but instead of a fault at the *east* end of the section, it is near the Housatonic River toward the *west* end (see fig. 10), and east of the fault there extends for three miles a single low synclinal of gneiss, with overlying quartzite at the west end and underlying quartzite at the east: and at the east end there is next—instead of a fault—the beginning of a low anticlinal, whose middle part is occupied by the southern continuation of Muddy Brook Valley, and whose eastern side is a bluff of quartzite overlaid by gneiss, and underlaid by alternations of limestone, gneiss or mica schist and quartzite.

The following are the details with regard to this Great Barrington section.

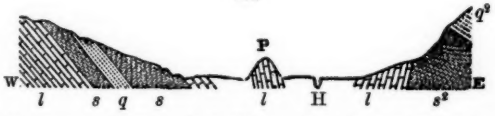
In my last paper on this subject, in volume v, of this Journal, I give, on page 88 and beyond, some account of the section *west* of Great Barrington, continued eastward through the village to the mountain (East Mountain) bounding the valley on the east, and illustrate it by the following cut (fig. 10). Here the low portion is the position of Great Barrington village, underlaid

* Page 265. The rocks of Monument Mountain owe their preservation as a prominent feature in the region to the cap of hard upper quartzite.

† This Journ., III, v, 48.

by steeply-dipping limestone; the low ridge on the left (west) consists of mica schist (and gneiss) with some intercalated quartzite (*q*), and, on the west side as well as at top, of the

10.



Section across the Housatonic Valley through Great Barrington.

Stockbridge limestone—all conformable to the Great Barrington limestone; the elevation on the east—East Mountain—is made of very firm fine-grained gneiss *unconformable* to the limestone adjoining, with a bed of quartzite at top 100 feet thick. H is the position of the Housatonic River.

Speaking in my former paper of the Great Barrington limestone, I observed that I had been unable to find evidence of its being a continuation, in a fold, of that to the west, and that I therefore supposed it to be a second independent stratum. The importance of settling this point has led to the delay in the publication of this continuation of my memoir. Having been recently over the ground again, I have finally arrived at the conclusion that the ridge of schist west of Great Barrington is actually the course of a synclinal, and that therefore the Great Barrington limestone stratum dips under it and is nothing but the Stockbridge stratum, or the same that covers the great plains of Egremont, Sheffield, Stockbridge, etc. The ridge of schist fades out half a mile south of the center of the village, and has low meadows south of it connecting with those of the river on the east and the limestone plains on the west; and the ridges which rise just south and continue the line southward are all limestone; so that there is no outcropping schist found to the south of it. Again, the connection of the quartzite of this ridge with that of the south end of Tom Ball, explained on pages 87, 88, of vol. v, and the fact that Tom Ball is itself the course of a synclinal, sustains the above conclusion. Hence I now believe that there is in the region no second or upper limestone stratum.

I pass now to the region east of Great Barrington.

The ridge begun in East Mountain (at the east or right end of the preceding section, fig. 10) continues for three miles (with a partial interruption at one place), to Muddy Brook Valley, where stands "Three-mile Ridge" as its terminus. This Three-mile Ridge is in the same line with Monument Mountain and is connected to it by a low ridge, and the whole together forms the west side of the open Muddy Brook Valley, while the much higher Beartown Mountain Ridge forms the east side.

(The map, in volume v, may be consulted for the positions of these points.) The south end of Beartown Mountain stands on a high plateau, 150 feet or more above the level of Muddy Brook; and where this plateau faces Muddy Brook Valley with a bluff front is Devany's hearthstone quarry (D Q on the map), an important geological locality referred to below. Three-mile Ridge stands directly opposite, on the west side of the valley (as represented in fig. 11).*

Along the high region from East Mountain near Great Barrington to Three-mile Ridge on Muddy Brook Valley, the rocks, gneiss and quartzite, form a very shallow synclinal. The dip at the west end of the section (right end of fig. 10) have at first a high dip, or 50°; but it changes in the course of 30 yards to 40°; and then a mile beyond becomes nearly horizontal; and in Three-mile Ridge, the low anticlinal which spans Muddy Brook Valley is already begun, its rocks dipping 10° to 15° to the northwestward. The quartzite at the top of East Mountain—a stratified and rather soft quartzite, in some places gneissoid—disappears to the eastward. In Three-mile Ridge the rock above (see fig. 11, left end) is the hard gneiss that prevails to the westward; but the lower part is a very hard, well-characterized quartzite, finely exhibited in the bluff front constituting its foot, but rising to a higher level to the southward. The fact that the formation of quartzite in preference to gneiss, or the reverse, depends only on very small changes in currents or levels is beautifully shown in the bluff; for near the middle of the hard and pure quartzite there is a layer of mica schist ten inches wide which to the southward commences to have a thin center of quartzite, and then, in the course of a hundred feet, is changed wholly to hard quartzite, while in the opposite direction (northward) it retains its mica schist character, but narrows gradually to six inches before passing under cover of the soil.

The rocks in the low synclinal between Three-mile Ridge and Great Barrington are then (1) the lower quartzite, q^1, of Monument Mountain (fig. 8), at the bottom of the front of Three-mile Ridge (fig. 11); (2) the gneiss, s^2 (of the same section), overlying this quartzite and making the mass of the high land; and (3) the upper quartzite, q^2, at the top of East Mountain at the west end of the section.

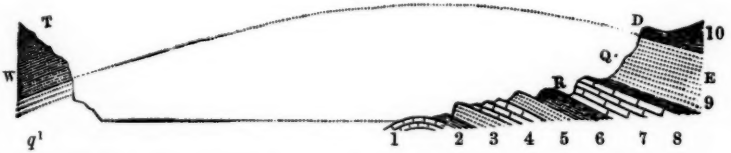
Three-mile Ridge, as I have said, and as the section in fig. 11 shows,† fronts on the limestone valley of Muddy Brook; and across, and directly opposite, is Devany's quartzite bluff, Q.

* Muddy Brook has its head just north of this point; but the broad valley continues on southward, and becomes the valley of the Konkapot, and at the same time expands eastward, over the lake region of Monterey.

† The diagram here given, to be true to nature, should have the dotted line representing the meadow land of the valley doubled in length.

In this bluff, and in the fields adjoining the road (R in figure) just south and southeast of it, there occur the alternations of limestone, quartzite and mica schist or gneiss, already alluded

11.



to. This interesting place—about four miles east of Great Barrington village—was made known to the writer by Mr. R. P. Stevens, of New York, who, many years since, studied the alternations of the rocks and made a section of the region.

The succession is as follows, commencing below, or in the field to the south of the road (R).

1. White granular limestone—that of the valley.
2. Mica schist, a thin bed.
3. Hard jointed QUARTZITE, 30 feet.
4. Same, limestone, 60 feet.
5. QUARTZITE, like the lower, 20 feet.
6. Gneiss, 30 feet.
7. (North of the road, R) bluish granular limestone, crumbling.
8. Mica schist, 6 to 8 feet.
9. QUARTZITE, 100 feet (Devany's quarry).
10. Gneiss (overlying the quartzite at the quarry).

The dip of the beds south of the road (R) is 30° to 35° to the eastward, the strike being N. 10°–30° E. But at the quarry, which is two hundred yards to the eastward, the dip is 20° to 30° to the northeastward. The change of strike and other facts, including the northwestward dip in Three-mile Ridge, show that the low anticlinal opens widely to the south: —the axis being inclined to the northward.

The alternations of quartzite and schist evince the little genetic importance of the distinction between quartzite and schist in Berkshire; and the intercalations of beds of limestone indicate that the conditions for sand or mud accumulations intermitted with those favorable for the shells or corals, or other material, fitted for limestone-making; and hence, that the area of sand depositions changed easily to an area of mud depositions, or of clear waters and abundant life. The lower of the quartzite beds (No. 3 in the section above) has but little lateral extent; for only a hundred yards off in the direction of the strike there is nothing but limestone. The old seas were hence much like modern ones in the irregularities of the bottom and in the outlining of sand-banks.

The thickness of the beds of quartzite and gneiss (or mica schist) between the lower schist (No. 2) and the upper (No. 10) taken together, is very near that of the lower quartzite (q') at the west end of the Monument Mountain section (fig. 8), and corresponds to it. Thus that quartzite which is there all quartzite, and in other places all mica schist or mica slate, here consists of alternations of the two, with only the upper half all quartzite; and, moreover, instead of including near its base merely a bed of calcareous quartzite, as in the west side of the ridge between Van Deusenville and Williamsville* there are three thin beds of the ordinary granular limestone.

Devany's Bluff has been stated to be the front of the plateau on which the south end of Bear Mountain stands. Going eastward a mile and a half, and then rising by a rough and steep road to the top of this plateau, the dip of the upper gneiss becomes northwestward, and the limestone formation outcrops on the plateau from beneath it and is the surface rock to the eastward over a large region.

Turning northwestward toward Beartown Mountain, the overlying gneiss is again reached; and the very small dip, 10° to 20°, and that to the northward, together with other observations in the region adjoining, show that the rocks in the southern end of Beartown Mountain, which are all nearly horizontal in position, constitute a shallow synclinal. Hence east of the low anticlinal spanning Muddy Brook Valley, a little to the north of its line, there is a shallow synclinal like that west of it.

But Beartown Mountain, while having nearly horizontal rocks at its south end, has its beds very steeply upturned to the north, and hence in that direction it is a *deep* synclinal. It is hence like Tom Ball* in being a shallow synclinal at one end and a deep at the other, but the shallow is at the opposite extremity—the south.

The rocks of the mountain all overlie the Stockbridge limestone. They are mainly varieties of firm and well characterized gneiss. Fissile dark-gray gneiss, approaching mica schist, and a firm less schistose variety of similar color, are common. But these vary on one side to a white feldspathic gneiss containing a little black mica, in short interrupted lines; and on the other side to a quartzose variety, which on weathering becomes at surface in appearance a curiously pitted quartzite. Again, the gray gneiss at the north end is an exceedingly firm gnarled rock, owing to the contortions experienced in the period of bold upturning and metamorphism. South of the railroad half a mile west of South Lee, immense blocks of this gnarled gneiss lie like ruins on the slope of the adjoining ridge.

* This Journ., III, v, 85.

In some places in Beartown Mountain there are hornblendic layers; and at one ascent of the mountain made from the west side, about midway between the north and south extremities, I found, near the top, a dark-gray, fine-grained layer, exceedingly tough, consisting largely of quartz and massive garnet. In a spur just east of the south end I observed in the gray gneiss a seam three-fourths of an inch wide of magnetite.*

I have stated that the rocks of Beartown Mountain overlie the Stockbridge limestone. Precisely the same kinds of gray, white, and quartzitic gneiss I found constituting the first ridge south of the road that leads up the valley to Washington from near Lenox Furnace. The Stockbridge limestone makes the base of the ridge and outcrops at its western foot; and the marble quarries of Lee are the same stratum two miles to the southwest.

Leaving now the region of the preceding section and Beartown Mountain, we may go west to the low north-northwest and south-southeast ridge that extends from Three-mile Ridge north to the south end of Monument Mountain, and follow the ridge northward.

First, at the notch where the road from Great Barrington passes through Three-mile Ridge, the gneiss is nearly horizontal, with a slight dip, where distinct, to the westward, as in Three-mile Ridge just to the south.

A mile and a half to the north of the notch, a rocky bluff faces Muddy Brook Valley, the rock of which, a hard gneiss, dips westward 10° to 15°. For the rest of the way north to Monument Mountain the ridge is mostly under soil; and along the western slope all is earth-covered. Toward the base of the western slope there are immense numbers of quartzite masses which seem to indicate the presence of a layer of quartzite beneath. As the few dips observed correspond with that in Three-mile Mountain, the ridge is probably, like that, the course of a shallow synclinal, but a narrow one.

Reaching the notch along which the Stockbridge road passes the south end of Monument Mountain (near the east or right end

* I speak, p. 262, of "rising by a rough and steep road to the top of this plateau." Near the point there reached, in the limestone area north and west of the road, I found a loose mass of white limestone consisting mainly of what appears to be rhodophyllite (rose-colored pyrosclerite), but containing also yellow chondrodite in grains; and, later, another smaller loose mass consisting of quartz and dark-green crystallized chlorite, along with large imperfect crystals of a remarkably beautiful rose-colored sphene. Since the first discovery I have been twice over the region in order to find the original site of these specimens, but without success. They suggest the suspicion that there may be an outcrop of Archæan (Laurentian) rocks in the vicinity. But in three ascents of Beartown Mountain, and an excursion along its whole western side, no evidence of any was met with. They may be of drift transportation. Until the minerals are found there in place, I forbear to draw the conclusion—seemingly the most probable—that they are from the rocks of the region.

of the annexed section, fig. 13), there is an exhibition of quartzite under its various contradictory phases. In the first place, on the Monument Mountain side of the road, stands out in some

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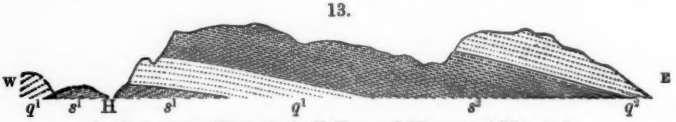


Section across Housatonic Valley and Monument Mountain.

places the intensely hard and enduring quartzite of the mountain, having joints, as usual, but only fallacious signs of stratification. On the opposite side, the rock, when any in place is seen, is also quartzite; but, unlike the mountain kind near by, it is, in the first place, very distinctly stratified; and, in the second, part is so soft or unenduring that much of it has fallen to loose dirty sand. Thin layers of a hard cellular quartzite often stick out from the sand, showing that there are strange alternations in the rock. What is stranger yet, there are also isolated portions, some 10 to 20 feet across, that are intensely hard quartzite, without a trace of lamination, except in some parts of the contour—the layers of which they originally consisted having been locally solidified into the hard massive structureless rock. But for evidences of transition in some parts of their contour these masses might be taken for transported boulders. The dip of the layers of this soft quartzite is 30° to 35° to the eastward, and the strike N. 5° to 20° E.

At one place, 200 yards to the north of the other outcrops, this soft quartzite occurs on the *west* side of the road with the dip reversed or to the westward, the amount 15° to 20°, and with the strike N. 20° W.; and here also there are immense boulder-like masses lying alongside of the crumbling kind, or seemingly imbedded in the soft quartzite. Such a quartzite easily changes to earth and thus makes a region of pseudo-boulders; and this suggests the true explanation, as I think, for the boulders on the west side of the ridge south of Monument Mountain, and shows that all quartzite boulders elsewhere are not drift boulders.

This soft, bedded, rotting quartzite at one place is exposed within forty feet of the massive enduring Monument Mountain quartzite, and with nearly the same dip, the latter not relenting in the slightest as it approaches the former. Are the two, notwithstanding the abrupt transition in texture, parts of the same layer? This is possible. Along the path descending northward from the eastern quartzite summit of Monument Mountain, the quartzite, elsewhere of extreme hardness, is for some yards thin bedded and rather fragile; showing a rapid falling off toward

the soft variety. Again, on the south and west parts of the mountain, the same bedded variety in some places occurs. Still, in each of these cases the passage is rather gradual and not absolutely abrupt, as here on the Stockbridge road.

In view of all the facts, it seems most probable that the soft quartzite at this place is really that of the lower quartzite stratum (q^1), and therefore that there is a fault near this east side of the mountain, along which the lower quartzite is brought up to a level with the upper quartzite (q^2). This conclusion is favored by the fact that a mile farther to the north, if not here, a fault actually exists east of the vertical eastern front of Monument Mountain; for the limestone of Muddy Brook Valley outcropping within 200 yards of the foot of the precipice (at *q*, on the map in vol. v), has a nearly vertical dip (75° to 80° to the eastward), although the eastward dip of the beds of the mountain is only 15° to 25°—the strike being N. 15° W. Moreover the stratification of the limestone is mostly indistinct owing to the soldering together of the layers in consequence of the lateral pressure attending metamorphism. The evidence proves at least that there exists here either a fault or a very abrupt synclinal.

The limestone at this outcrop is the Stockbridge stratum; it outcrops at several points along the way to Stockbridge and Glendale; and it is consequently the same stratum that is seen descending under Monument Mountain at its west end near Housatonic village.

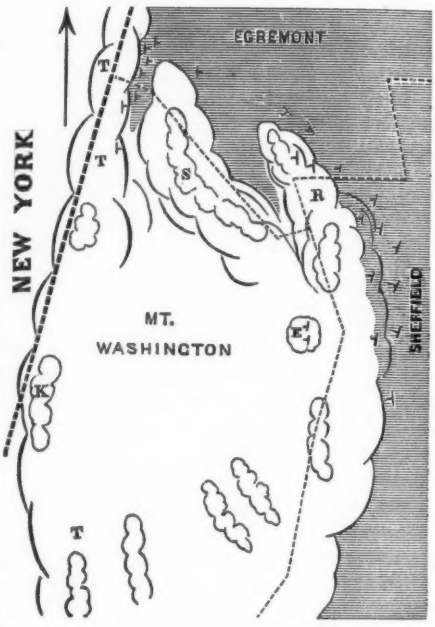
The porosity of many of the hard layers in the soft quartzite is probably owing to the former presence of disseminated carbonate of lime; and the ready disintegration of the other layers may be due to the same fact. Yet the soft quartzite crumbles in other localities without such a cause. The thin-bedded porous quartzite often has on its surfaces minute brown tourmalines.

4. *Observations outside of the Great Barrington Region.*

a. *Mt. Washington and Western Sheffield.*

Mount Washington is a mountain plateau, situated in Southeastern Massachusetts, with its western side in New York and southern in Connecticut. It is about 2,000 feet in elevation above tide level, and four to five miles across at top. Its western side stands in the line of the Taconic Range; but the mass of it projects to the eastward, and is almost wholly within the State of Massachusetts. Mount Everett, the highest point, —2,634 feet high, according to the Massachusetts triangulation,—is on the eastern border. It has the limestone plains of Sheffield on the east, and those of Egremont on the north.

This projection of the mass from the east side of the Taconic Range is shown in the accompanying outline map of the region. T T is the course of the Taconic Mountains, situated along the boundary between N. York and Massachusetts; while E, more than three miles east of the boundary, is the position of Mount Everett. The part lined over is the limestone region lying to the north and east and covering a portion of the eastern foot of the mountain.



R and S are two ridges extending from the plateau northward into Egremont. The limestone reaches into the intermediate valleys, even into the narrow western, between S and T (the one by which the carriage road leads to the summit of the plateau on the way to Mt. Everett, or to Copake).

The rock of the mountain, and also of the ridges R, S, T, is a fine-grained mica slate approaching (if not really) hydromica slate, and much of it is chloritic. It is the same that constitutes Tom Ball near Williamsville, west of Housatonic, and various ridges of the Taconic Range; at the summit of Mt. Everett it is much seamed with quartz.

Mount Washington is a synclinal with limestone below and slate above. In the plains of Sheffield, near the eastern foot of the mountain, there are two parallel north-and-south roads nearly a mile apart. Along the more eastern the limestone has its usual easterly dip—the amount 35° to 45°. But along the western—the under-mountain road—the dip is *to the west* 25° to 30°, with the strike N. to N. 25° E. (compass course). The limestone extends up the slope of the mountain for 100 to 120 feet above the road (by estimate, 700 feet above the sea level), and retains throughout its westerly dip; and then there follows the overlying slate of the mountain, with the same dip. This plunge

of the limestone westward under the slates of Mount Washington may be traced along for four to five miles, even to the north extremity of the ridge R; and it characterizes also the high eastern summit, Mt. Everett, where the westward dip is 35° to 60°. East of the extremity of the ridge R, the strike of the limestone has some westing, becoming first N. 15° W., and then N. 30° W., and the last is the strike just north of it.

The limestone thus dipping under the mountain does not emerge short of the region of Copake in New York, and consequently the Copake and Sheffield limestones are the same stratum—both, the Stockbridge limestone; and the limestone stratum, therefore, makes a synclinal.

But the slates of the ridge S, and also those of the Taconic Range, T T, dip *eastward* 45° to 70°.

Moreover, the limestone of Egremont west of the ridge R takes the steep easterly dip of the ridge S, and the same also is the dip of the extension of it up the valley between S and T, where there are fine exhibitions of the conformability of the slate and limestone—the dip being there 60° to 70° and the strike nearly north and south.

We find thus evidence of a very broad synclinal across the center of Mount Washington. But just north, in Egremont, the structure is totally different; the ridges S and T are the courses of very steep and comparatively narrow independent synclinals, with the axial plane inclined eastward.

We learn then that the trough or synclinal, in the formation of which was involved the making of the mass of Mount Washington, commenced three times farther east than that for the Taconic Range to the north of it; and that thus the mass of enclosed slate is more than three times as wide, and hence it happened that the mountain there made was a lofty one. The lateral pressure, from the eastward, beginning the downward trend a mile east of the foot of Mount Washington, made a trough whose bottom on the east inclines westward at a comparatively small angle, as shown not only in the slates seen near the eastern foot but also in those of Mt. Everett; but over the corresponding region just north, in Egremont, the displacements were small and no mountain synclinal was begun until near the site of the Taconic Mountains; and there the whole force acted on a relatively narrow trough, making it of great depth, and pressing it over until its axial plane, like all the slate rocks enclosed, dipped eastward 50° to 70°. Then, quite near Mount Washington, the second synclinal, S, was made with the same steep inclination. The synclinals S and T become merged in one mass, in Mount Washington; and as the limestone does not appear at the summit, the intermediate anticlinal in the mountain was only an anticlinal of slate. In other words, the syn-

clinal of limestone beneath the mass of the mountain was one great trough with breaks and incipient flexures; while to the north these incipient flexures became two defined synclinals, with the intermediate anticlinal — the synclinals being the courses of the ridges S and T, and the anticlinal that of the limestone outcropping between; and then, farther north there was formed the Taconic synclinal T alone. West of Southern Alford the Taconic slate and limestone have the same steep eastward dip.*

It is to be noted that a limestone as soon as made is as hard and unyielding as at any time afterward. It is hence far more resistant to flexure than the slates. This will account for changes in the flexures within short distances. The limestone of Sheffield and Egremont generally dips eastward at an angle as high as 35°, showing universal displacements; but where are its faults and folds it is for the most part difficult to discover.

The Taconic Mountains hence consist of no older rocks than Monument Mountain, or Tom Ball, and the multitudes of other heights in Berkshire, which are made of strata overlying the Stockbridge limestone. Further, the gray, white and quartzitic gneiss of Beartown Mountain, and the mica schist and quartzite of Monument Mountain, are strict equivalents of the chloritic mica slate of Tom Ball, Mount Washington, and the Taconic Range. It will be remembered that Tom Ball is only two miles from Monument Mountain, and but one from a ridge of the quartzite.

If then the Stockbridge limestone is of the age of the Chazy and Trenton, the *Taconic slates* may be reasonably set down as of the Hudson River or Cincinnati group.

To the eastward of Mount Washington, and half to three quarters of a mile west of the village of Sheffield, there are two narrow ridges of mica schist, each about half a mile long, standing surrounded by limestone. The western, called Bear's Den Ridge (from a deep gorge in its west side), is about 150 feet high, above the plain around; and the other, Barnard's Ridge (so named after President and General Barnard, whose paternal residence is near by), about 120 feet. The schist at Bear's Den and the limestone adjoining dip 60° to the eastward. The western side of the ridge is limestone, and the wearing away of the limestone at the junction has made the gorge, while the tumbling of great masses of the schist has contributed greatly to the wildness and beauty of the place. Each of these ridges is a local synclinal of limestone and schist; the trough of limestone was small, and hence it pinched in but little of the overlying schist. The layer of the schist folded together in the synclinal was probably not over 80 yards thick,

* This Journal, III, v, 86, 91.

making the whole thickness of the schist between the jaws of limestone about 160 yards. The schist involved in the synclinal is but a very small part of the great formation that overlay the whole limestone region when the folding began; for the thickness in Mt. Washington, between the top of Mt. Everett and the underlying limestone, is not far from 1000 yards, and this is what is left after ages of denudation.

The mica schist of Bear's Den is a coarse kind, full of translucent garnets a tenth of an inch in diameter, and *some layers abound in dark brown slightly translucent crystals of staurolite.* The staurolite crystals have the planes *O*, *I*, 1-$\bar{1}$, and the garnets are dodecahedrons with the edges deeply truncated.

In the limestone of West Sheffield, two miles east of Mt. Washington, an opening has been made by Mr. Ralph Little of Sheffield, which has afforded some copper pyrites, with traces of galena and blende, and also rutile in crystals one to two or three inches long and an eighth to a fourth of an inch broad.

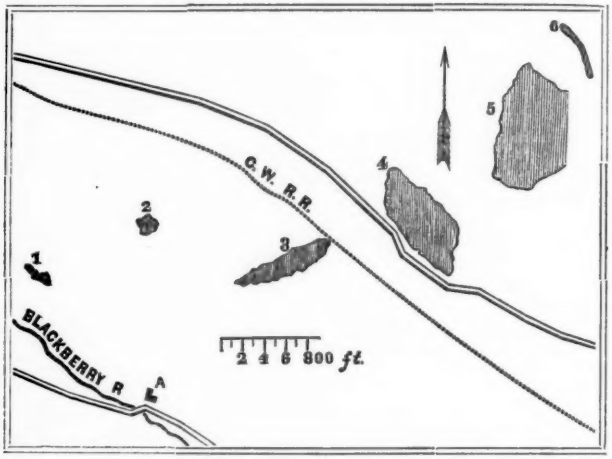
b. *Canaan, Connecticut.*

From the region of Great Barrington, the mica schist (or gneiss) and quartzite extend southward to Sheffield, and thence through East Sheffield to Canaan, the hard quartzite constituting long ridges the latter part of the way. At the same time, the great limestone formation of Great Barrington and Egremont stretches south to the same town, with undiminished breadth; there is hence no doubt that Canaan and Great Barrington are within the same geological region.

The patches of quartzite at Canaan, taken by themselves, or even with outcrops of some other localities, suggest strongly and enforce the conclusion presented in my former paper;* for the isolation of the areas with limestone around, the absence of stratification, and the apparent uncomformability and independence, are as I have stated. But, in view of the later observations—that the hard jointed quartzite may pass in the course of a few rods into soft friable quartzite; that it may graduate almost as abruptly into mica schist or mica slate, either firm or decomposable; that the hardest and most massive kind may directly overlie the crystalline limestone, or even be interstratified with it, as well as with the mica schist or gneiss, and the latter not participate in the system of joints that pervade the other; that small beds of quartzite may lie isolated in the upper part of the limestone formation; that consequently, the qualities, thick or thin, of wide or narrow extent, mean nothing, as applied to the hard quartzite—the facts are not at all adverse to the views gathered from the Great Barrington region.

* This Journal, III, iii, 179.

The only place in the outcrops of quartzite near Canaan, where the stratification is distinct, is in the northeastern of the patches, Nos. 5 and 6. (I reproduce here the map of the region, prepared for me by Mr. Adam.) I still find, on a re-exam-



ination of the region, the uncomformability of strike between the quartzite of No. 6 and the limestone outcrops but a few rods distant strongly marked; and, moreover, directly in its line of strike, but a hundred yards off, there is nothing but limestone. Still, there is much variation in the strike of the quartzite itself, the angle being N. 60°–65° W. at the northwest end, N. 45° W. at the middle, and N. 30° W. at the south end; and much also in that of the limestone at its parallel outcrops 20 to 25 yards to the westward, that to the northwestward being N. 80° W. and N. 70° W.; while only 12 yards west of the southeast end of the quartzite, the dip of the limestone is N. 35° W. Now, noting, in connection, that limestone uplifts are very irregular in their strikes, and also the fact that to the eye, from a little distance, without the use of an instrument, there is apparently a general conformity in the dip of the quartzite and limestone, we may conclude that the limestone does dip conformably beneath the quartzite (No. 6). The fact that there is only limestone to the northwest, in the line of the strike of the quartzite, is proof that the bed is one of small lateral extent, like one observed near Devany's quarry,* or that there is a fault. Seventy-five yards farther to the northeast, the limestone outcrops again, with the strike N. 53° W., and dip to the northeastward as in the other

* No. 3, page 261.

cases, as if it were a bed overlying quartzite No. 6; and this it is, unless there is a fault between the two. The quartzite of patches Nos. 5 and 6, having the dip in opposite directions, may be parts of one bent layer; for the thinness of the bed No. 6 may be only the thinness of the *hard part* of the bed, a soft upper portion having been removed. Hence, while my previous deductions are sustained by Canaan facts, they are wrong only because facts elsewhere observed have proved that the quartzite has no fixed qualities, and is hence the most deceptive of rocks.

Rattlesnake Hill, just north of Canaan, while consisting mainly of a decomposable arenaceous mica schist, has below a stratum of soft quartzite over the limestone. Canaan Mountain, a very prominent feature in the topography of the town of Canaan, rises from the plain half a mile or so south of the above quartzite outcrops, and *stands on a high base of limestone*, outcrops of which are best displayed on the southwest side. The rock of the mountain, over the limestone, four hundred feet or more in thickness, is mainly coarse and fine mica schist, passing into gneiss. I have not found any quartzite stratum, but it is possible that the soft kind may be concealed beneath the high grassy slope of the north side. *Canaanite*, or white augite rock,* occurs in many places in the upper part of the underlying limestone.†

c. *South Canaan and Falls Village.*

The village of South Canaan is situated to the south of Canaan Mountain, and two miles east of the Housatonic River, where is Falls Village and the railroad station. A few rods to the southeast of the South Canaan Church, there stands what may be called a spur of Canaan Mountain, named Cobble Hill. This hill consists below, to the west, of quartzite, dipping southeastward 40° (strike about northeast). Above the quartzite, 20 to 25 feet of which are exposed above the road, there is a stratum of mica schist and thin-layered gneiss, containing scales of both black and white mica; and above this, and constituting the body of the hill, there is a whitish granite-like gneiss, great blocks of which lie at the foot of the bluff adjoining the road. The quartzite is mostly the soft

* Both tremolite and white augite occur in the Canaan region. The Canaanite is a massive form of the latter, as first observed by Prof. C. U. Shepard, in his Report on the Geological Survey of Connecticut, 8vo, 1837, p. 135.

† In the observations on the rocks at Poughquag (this Journal, III, iii, 255) it is stated that the quartzite and the limestone at outcrops 200 yards apart are unconformable. It is quite possible that within this 200 yards, where the rocks were concealed from view, there is a bend in the limestone, bringing it into conformity of dip with the nearly horizontal quartzite, and that thus it may pass conformably beneath the latter rock; and facts elsewhere observed render this most probable.

bedded kind, and in its upper part it contains, at intervals, some streaks of black and white mica, thus showing its relations to the overlying schist. The dip of the rocks conform to that of the limestone not far distant, and there is no reason to doubt that the quartzite, with the other crystalline rocks, overlie conformably the limestone.

At Falls Village, the limestone outcrops above the river at the falls and over the plains adjoining. Just above the falls, on the west side, there is an outcrop of well characterized mica schist; and half a mile to the northwest the schist forms a ridge, with limestone outcropping in its lower slopes from beneath the schist, both rocks having a dip of but 15° to 20°. The mica schist contains small garnets, and some of its layers *abound in crystals of staurolite.* This locality of staurolite was mentioned to me by Dr. Stephen Reed, of Pittsfield, as well as that at Bear's Den in Sheffield.

d. *Rutland, Vermont, and Graylock in Northwestern Massachusetts.*

Vicinity of Rutland.—I have not attempted to give the Rutland region a careful study, but have made some observations of interest bearing on the relations of the same to that of Great Barrington.

On the eastern border of the Rutland limestone, just east of the road from Rutland to Mendon, there is a portion of that long Vermont range of quartzite, which is marked on the geological map of Vermont as extending from the Massachusetts boundary to a point just north of Mendon, sixty miles, and thence, after a brief interruption, to Rockville, nearly forty miles beyond. This so-called "quartzite" ridge consists, near Rutland, of chlorite slate containing magnetite, and hydromica slate (the so-called talcose slate or talcoid schist), along with quartzite; and, as has been stated (this Journal, III, iv, 69), the hydromica slate graduates laterally by rather abrupt transitions into the quartzite, the same bed being quartzite in one place and hydromica slate 100 yards off. Part of the quartzite looks like gneiss, owing to streaks of silvery mica (hydrous mica?) in largish scales. Another part is a coarse conglomerate.

The rocks in the Rutland ridge, excepting the massive quartzite, are well stratified, and have a dip to the eastward corresponding with that of the Rutland limestone and apparently lying over it. I found no vertical section of quartzite and limestone near Rutland, showing positively that the quartzite with the hydromica and chlorite slates were uppermost. But the dip of the rocks at the nearest outcrops was the same with the limestone; and at one outcrop of the limestone, the

upper layer was covered above with a coating of quartzite, confirming the conclusion that the quartzite is the superior rock.

It will be observed that the variations from hard to softer quartzite, and from quartzite to metamorphic schists, are similar to those observed in Berkshire. East of Brandon, a mile or two beyond the limonite, lignite and kaolin bed of "Forestdale," I visited another part of this Vermont quartzite range. There the rock is of the hard jointed kind. But the bedding is in some places distinct, and in a few layers there are slaty portions.*

Graylock.—Graylock, standing between South Adams and Williamstown, in Northwestern Massachusetts, 3600 feet above tide level, while Taconic in system and in the character of its rocks, belongs to a parallel line of elevations about six miles east of the Taconic Ridge. In structure and origin it is closely like Mt. Washington. This is shown as far as relates to the existence of the synclinal, in the section of it published by Prof. Emmons, at page 19 of his American Geology (8vo, 1853), the general truth of which I have verified.

This section commences at South Adams, and terminates at the entrance to the great funnel-shaped valley called the Hopper, where the limestone stratum dips *eastward* under the mountain. The dip of the slates at the summit of Graylock and to the west of it is steep to the eastward.

At the narrow north end of the Graylock range of elevations, abreast of North Adams (5½ miles north of South Adams), the limestone and slate *both dip eastward*, just as with the Taconic ridge in Egremont north of Mt. Washington; and the explanation is, I believe, the same: that the broad synclinal which holds the broad and lofty mountain, is at this place pressed to a steep and narrow synclinal with the axial plane inclined eastward. The very large and deep mountain-girt depression called the Hopper, north and south in trend, suggests strongly the view that two synclinals with an intermediate anticlinal exist in the slates of the broader part of the mountain mass, and that the existence of the Hopper was determined by the position of the anticlinal; while the limestone synclinal may have been a nearly simple one beneath the slates. My own observations over the region have been too limited to make this more than a suggestion.

* The Geological Report of Vermont makes the quartzite east of Brandon to extend west over two miles of country which is without outcropping rock and deep in earth, to the bed of limonite and kaolin at "Forestdale." But the kaolin has come from the decomposition of feldspar; and it is probable that a decomposable mica or hydromica schist underlies the earth-covered interval referred to, and that its feldspar was the origin of the kaolin.

CONCLUSIONS.

I close with the following conclusions as a sequel to those mentioned on former pages of this memoir.

XIII. *Lithological evidence as a test of Geological age.*—It is above shown that the mica schist overlying the Canaan limestone is in Sheffield and Canaan *staurolitic. The mineral staurolite exists, therefore, in rocks younger than the Stockbridge limestone.*

But the facts which have been presented, besides opposing the idea that staurolite is a satisfactory mark of age, also show that lithological evidence should be used with extreme caution even within a district of a few square miles. For in a distance of three miles or less, a stratum of quartzite has been found to become one of mica schist, or of gneiss, or of mica slate, garnetiferous or not, or of hydromica slate, or of chloritic mica slate, or chlorite slate containing magnetite or not; and in another locality, in the course of two miles, the quartzite stratum changes to a coarse staurolitic mica schist, full of garnets; and in another, mica schist is replaced by a granitoid gneiss. Had I known these facts when I commenced my investigations in Green Mountain geology, I should have had much less difficulty in reaching right conclusions. It is hence evident that, in the present state of the science, we should beware of all conclusions as to the age of metamorphic rocks that rest on lithological evidence alone, whether they relate to præ-Silurian or subsequent time. When every square mile of New England has been investigated *stratigraphically* with thoroughness, we shall begin to know something of its geology.

It should be understood that tracing a formation along from mile to mile by means of the rocks, *after an understanding of their characters and transitions*, is a very different thing from deducing the age of rocks on the principle that a particular kind belongs to a particular age in geological history.

It follows from the facts reviewed, as stated in an early part of this memoir, *that there is not strictly any quartzite formation in the Green Mountains; the formation being made up of various rocks, and quartzite being not always the predominant one.*

XIV. *Age of the Stockbridge Limestone.*—This mass of limestone, as I know from personal study, extends continuously from Pittsfield to Canaan.* Beyond Canaan it stretches on, as shown by Percival, to a point south of Pawling in Dutchess Co., New York—making for the total from Pittsfield southward a length for the formation of 70 miles. To the north of Pittsfield, I have examined the limestone only at isolated points. At these I have found no reason to doubt the conclusions of the geologists of Massachusetts, Vermont and Canada, that the

* The one interval not examined when page 52 of volume v was printed, I have recently been over.

rock, all the way to Rutland 80 miles from Pittsfield, and 20 miles beyond, is the same limestone. The evidence consists not only in the apparent continuation and similarity of the limestone throughout, but in the fact that with very small interruptions the quartzite continues to accompany the limestone, especially along its eastern border, just as in Berkshire, and that it undergoes the same transitions to mica schist, mica slate or hydromica slate, along its course; and, further, that the mica slate accompanying the limestone, especially along its western border, is very similar throughout. That the evidence should be conclusive, the actual continuation of the limestone through, from Pittsfield northward, should be ascertained, and also the stratigraphical relations of the quartzite to the adjoining limestone. What is now known is sufficient to render the conclusion exceedingly probable. If so, the age of the limestone at Rutland and other parts of Vermont is approximately that of the rock in Berkshire and farther south.

I have referred to the facts with regard to Chazy fossils found in the Rutland limestone by the Rev. A. Wing. As the facts are of the highest importance, the paragraphs may be here cited. (This Journal, vol. iv, 133, 1872.) Mr. Billings says—

I received last summer some fossils from Rev. A. Wing, and made the following note upon them before I sent them back.

15th June, 1871, from the Rev. A. Wing, twenty specimens with the following ticket: "Encrinites and obscure fossils, supposed to be Trenton, collected May, 1870, at the marble quarries, West Rutland, not one hundred yards northwest of an abandoned marble quarry,—the most northern one worked on the southwest side of the valley, say one hundred and fifty rods southwest from Barns' hotel, West Rutland."

This is the description of the locality given by Mr. Wing. My note on the fossils in the book I keep for such purpose is—

"These consist of numerous obscurely preserved forms like *Pleurotomaria staminea*, small *encrinal joints*, and a single plate of *Palæocystites tenuiradiatus*. I think this collection is Chazy. The rock is a gray fragile limestone with white crystalline seams in it."

If Hitchcock's Eolian is Stockbridge limestone, then the latter includes the Chazy. The plate of the Cystidean, *P. tenuiradiatus* Hall, is a never-failing guide to the Chazy; at least it is so on the west side of Lake Champlain.

That all the facts may be before the reader, I quote a page (pp. 418, 419) from the Vermont Geological Report on other fossils from the same limestone.

Although the Eolian limestone is largely distributed in our country, no fossils have been found in it, except the few which occur in Vermont. We have found the following genera, to which specific names cannot yet be attached: *Euomphalus*, *Zaphrentis*,

Stromatopora, *Chætetes* and *Stictopora*. Two kinds of encrinal plates have also been discovered. These genera were determined by Prof. James Hall.

Euomphalus. This is a genus of univalve mollusks, somewhat resembling Maclurea and Ophileta. It is most common in the Devonian and Carboniferous rocks, though not wanting in the Lower Silurian. This genus has been collected from the Eolian limestone in Whiting and Sudbury. A shell of the same genus is found in the Hudson River limestone in Orwell, which is quite near the locality in Sudbury. There are two species of Euomphalus in the Eolian limestone.

Zaphrentis. The three other genera are of corals. *Zaphrentis* belongs to the family of *Cyathophyllidæ*, and there is no instance in this country where this genus has been found below the horizon of *Atrypa reticularis*, which is an Upper Silurian and Devonian species. The two are found together in great abundance in the semi-crystalline limestone connected with clay slate upon Lake Memphremagog near talcose schist. We cannot ourselves distinguish between the specimens of *Zaphrentis* from Lake Memphremagog and the Eolian limestone. This genus occurs in a dark colored slaty limestone in Sudbury and Cornwall, associated with Euomphalus and Chætetes.

Stromatopora. This is the most common of all the fossils in the Eolian limestone, and it is a genus whose lower limit is in the Niagara group of Upper Silurian rocks. It has been found in Sudbury, Orwell, Cornwall, East Middlebury, Brandon, New Haven and Williston. The specimens from East Middlebury and Williston are referred to this genus with a query. There are probably two species of Stromatopora in the Eolian limestone.

Chætetes. This genus is both an early and late one—that is, it is found from the Lower Silurian to the Carboniferous rocks. Both branching and oval forms of this genus occur in the Eolian limestone of Vermont, and there are two distinct species of it. It has been found in Sudbury, Benson, on M. C. Rice's farm, Cornwall and Williston. A supposed fossil of this genus is quite common at Peck's quarry in Cornwall, and on Dea. Casey's farm; at New Haven on the railroad north of New Haven railroad bridge, and in a loose block of limestone at Middlebury village.

Stictopora. In Sudbury were also collected specimens of the genus *Stictopora*. This genus was remarkably well defined. Prof. Hall did not venture to pronounce upon the species. An obscure coralline form, distinct from any of the others described, has been found in the limestone above Sheldon & Slason's marble quarry, in West Rutland.

Encrinal stems. The large encrinal stems from Sudbury and Cornwall are interesting, because the only cases where encrinal rings of such dimensions have been found in this country are in Devonian rocks. The fragments of small encrinal stems that have been discovered in the Eolian limestone, were found at the marble quarries in East Dorset, upon Mt. Eolus, and in West Rutland, Sudbury and Cornwall.

One will be struck with the fact that all these fossils have an Upper Silurian or Devonian character, wherever they are of value in identifying strata. * * * *

We incline to the opinion that they must probably be placed as high as that formation, or as low as the Lower Silurian: to which last position Mr. T. S. Hunt assigns them.

With regard to the Vermont quartzite, we have the following facts from page 356, of the same report.

Several species of fossils occur in the quartz rock. They are a species of *Lingula*, a mollusk resembling the *Modiolopsis*, a straight chambered shell (?), a few crinoidal columns, the *Scolithus linearis* (Hall), a few fucoids, and some indeterminable forms which are evidently organic.

The Lingula is from the north part of the principal range of quartz rock in Starksboro, near Rockville, at the house of Mr. Hill. The locality was discovered by Henry Miles, of Monkton. The specimens contain scores of fossils, but none of them are very distinct. Prof. James Hall has examined them, and regards them as a new species of Lingula, related to a species contained in the Medina sandstone.

The "Modiolopsis," chambered shell, and encrinal remains are found in hyaline quartz on the west side of Lake Dunmore. The first and last were referred to the same authority by Prof. Adams.

From the above statements it would appear that the limestone is either later Lower Silurian, or younger. But species of Zaphrentis have been described by Billings from the Lower Silurian (Hudson River group, one of them three inches long and an inch and a half in diameter at top); and it has been inferred by Hall, Billings, Hunt and others that the beds are probably Lower Silurian. The encrinal stems, as I learned from Prof. C. H. Hitchcock, were not over three-eighths of an inch in diameter. Putting the facts together, the evidence is strong that the limestone is not older than the Chazy, and that the Trenton limestone is included with it, its deposition having followed that of the Chazy, as in most regions of Chazy rocks. Excepting the *Scolithus*, there is nothing in the quartzite that suggests a Potsdam origin, or anything older than the Hudson River group; and the Scolithus is of no particular age.

The results thus take us back to the old idea of the Professors Rogers, that the limestone is mainly Trenton, and therefore that *the slates of the Taconic Range are of the Hudson River group.* The two make up the true original Taconic system of Emmons.

Before leaving this topic I make one remark of a personal nature. Through the early part of the Taconic controversy—in which the principal contestants were Prof. Emmons on one side, and Prof. James Hall and the Profs. Rogers on the other—I took no part, as I had given the facts no investigation in the field.

At the same time I inclined toward the views of Profs. Rogers and Prof. Hall because of my confidence in them as geological observers. When, thirteen years since, Sir Wm. Logan, after investigations in the Green Mountains of Vermont and Massachusetts, as well as in Canada, brought out his view that the Taconic rocks—those of Taconic mountains and the associated limestone—were identical with the Quebec group,* (an opinion then participated in, I believe, by Prof. Hall,) I gave it acceptance and adopted it in my Manual of Geology. Within the past half a dozen years I have looked to the rocks themselves in order to obtain ground for an independent judgment; and the result is that above presented. It makes the Taconic rocks and the associated limestone one in formation, as held by Logan; and it differs from his view as to the age of the beds—on the basis partly of the older examination of fossils by Hall, and especially on the recent, by Billings; and diverges also as regards the relations of the quartzite.

The conclusion I have presented relates to the *original Taconic* as presented by Emmons in his New York Geological Report published (in quarto) in 1842, and not to the unfortunate additions to it made in his later publications†—unfortunate because they drew off attention from the real Taconic, and became the chief subjects of controversy and prolific sources of error.

XV. *The condition of the Green Mountain region in Berkshire during the formation of the rocks.*—The occurrence of the quartzite along the limestone region, and its passage occasionally into conglomerate, testify to the existence in the Green Mountain seas, during the later part of the Lower Silurian, of great exposed sand flats, such as are made in the face of the waves; and the frequent transition in the quartzite stratum to mica schist, or gneiss, or mica slate, and the like, are good proof that in the shallow waters about the great sand banks, the bottom was of mud. The fact that the quartzite prevails most along the eastern border of the limestone shows that in that portion of the region the water was most shallow, or that there lay the largest amount of emerged land. Whatever the depth of water in which the limestones were made, the depth appears to have diminished in the Berkshire region toward the end of the Lower Silurian era; and this was a prelude to the time of upturning and metamorphism which appears to have closed that era in Western New England.

It is a remarkable fact that the conglomerate accompanying the quartzite contains no pebbles of gneiss or of any of the older crystalline rocks. The pebbles are all of quartz or quartzite.

* The views of Logan are to be found in this Journal, II, xxxi, 210, 1861; and also, with additional conclusions, by T. Sterry Hunt, at page 392 of the same volume, in an article entitled "On some points in American Geology."

† See this Journal, III, iii, 468.

ART. XXIX.—*De Verneuil. Eulogy of* M. DAUBRÉE, *June* 4, 1873.

THE distinguished associate of whom the Academy has just been deprived, alike eminent in character and intelligence, affords us an example, too rare among us, of an independent position consecrated with zeal to science and crowned with important discoveries.

PHILIPPE EDOUARD POULLETIER DE VERNEUIL, born at Paris, Feb. 12th, 1805, was destined to the magistracy, and had attained to twenty-five years of age, when the events of 1830 put an end to his plans in that direction.

At the moment when he hesitated as to what pursuit to devote himself, geology was making marked progress. No only was it admitted that the crust of the earth, instead of having always remained unchanged, as the school of Werner demanded, had undergone foldings and fractures which revealed transformations in its structure, but it was becoming possible to determine the relation in age of these phenomena. It was under such circumstances that M. de Verneuil felt himself drawn toward geology, and that he followed with great ardor the lectures in which M. Elie de Beaumont developed these new ideas. The great questions connected with the history of the globe soon took strong hold of him, and he resolved not to remain a mere spectator of the discoveries of others.

Recognizing the fact that in geology, as in all the sciences of observation, the study of nature alone can give a full understanding of her phenomena, he desired to travel. He chose at first the country of Wales, which at that very time the investigations of two distinguished English geologists, Sedgwick and Murchison, were rendering classic, for they were establishing divisions and a fixed order of superposition in the great group of ancient beds which, up to that time, had been confounded under the general name of Transition formations. As has more than once happened, this first journey had a decisive influence upon the future direction of the researches of M. de Verneuil, and upon the character of the services to science by which he was to become distinguished.

His desire to see and compare soon led him to the east. He turned toward Turkey, following down the Danube, on which steam navigation was just then introduced, and, led on by meeting those who sympathized in his objects, he went through Moldavia and Bessarabia to Odessa in the Crimea, and even to the frontiers of Circassia, and later toward the Bosphorus. The memoir on the Crimea which he then published was accompanied by descriptions of new and interesting fossils described by

M. Deshayes. This savant, who now came to the aid of stratigraphical geology by his profound knowledge of fossil shells, took much pleasure in instructing de Verneuil in this important study in a private course rendered illustrious by distinguished auditors, themselves soon to become eminent geologists.

After having made, in 1838, a special study of the lower beds of the Bas-Boulonnais, M. de Verneuil had already become an authority in the determination of fossils. Thus, in 1839, when Sedgwick and Murchison wished to compare the oldest formations of the countries of the Rhine and of Belgium with those of England, they desired de Verneuil to accompany them in their explorations. Absorbed as they had been in the study of stratigraphical relations, they found the necessity of his cooperation, which was the more important to them since M. de Verneuil had already travelled over and studied these countries. In the memoir which they published, these English geologists made full acknowledgment of the assistance their companion had afforded them by generously putting at their disposition the rich collections he had made in person. In connection with M. d'Archiac,—whose name we cannot pronounce without recalling our great sorrow in the loss of this distinguished student, our most excellent associate,—M. de Verneuil published in 1841 a description of the fossils of the older beds of the Rhenish provinces. The work is preceded by a general sketch of the fauna of the so-called Paleozoic formations, and followed by a table of the organic remains up to that time discovered in the Devonian system of Europe.

This journey made apparent the advantage, I had almost said the absolute necessity, in order to carry out such explorations with accuracy, of having always at hand a trained paleontologist like M. de Verneuil. At this time he was in fact almost alone in his knowledge of fossils. So, when Murchison, desiring to study further the geological formation which he had so well defined in the northwest of Europe, formed the project of exploring Russia, he again sought the companionship of M. de Verneuil. The facility with which Murchison could at a glance appreciate the characteristics of strata could never in that way alone have enabled him, able as he was, to reach sure distinctions in a region so vast, where, moreover, the rocks are so seldom uncovered. These two savants supplemented each other in the happiest manner.

The labor of three summers, 1840 to 1842, enabled Murchison, Verneuil, and Keyserling, to explore a region comprising more than half of Europe. It is just to say that the Emperor Nicholas warmly favored this enterprise, the grandeur and utility of which he fully appreciated; and that many learned men, Russians and foreigners, had published papers on various

isolated parts of the region. Traveling by different routes, and meeting from time to time to compare their observations, the three savants gave a very wide range to the field of their operations. The nearly horizontal position of all the formations beyond the chain of the Urals is in marked contrast with the manner in which the same groups are upturned and broken in Western Europe; and the great extent of the beds favors a rapid acquaintance with them. The work upon Russia in Europe and the Ural Mountains, accompanied as is well known by geological maps representing each of those countries, appeared in 1845. It is a monument raised to the knowledge of the immense region to which it relates, and at the same time to the fundamental principles of geology. The introduction of the Permian system into science was one of the great results of this exploration.

As the conclusions rest entirely on the exact determination of fossil species, it was essential to give to this part of the work all the care and all the range which it required. The whole of the second volume, which contains, so to speak, the justifying testimony, is, in all that pertains to the Paleozoic fauna, the personal work of M. de Verneuil, assisted by Count Keyserling. The fauna of the secondary formations was intrusted to M. Alcide d'Orbigny, at that time the authority in this department of the science. Viewing together the four Paleozoic systems, the authors show that the succession of organized beings is almost exactly the same as in the other countries of Europe.

The various investigations which have been carried on with so much activity in North America have made us well acquainted with the wonderful development of stratified deposits in that part of the world, remarkable not only for the thickness of the beds, but also for their geographical extent, comprising not less than thirty-four degrees of latitude and fifteen degrees of longitude. But with wise independence, the American geologists did not consider, in laying down the subdivision, the groups that in Europe appeared to be their analogues; moreover, the data were entirely wanting for an exact comparison. When it is possible to follow deposits from one country to another without interruption, it is easy to trace out correspondances among them; but this cannot be done between two continents separated by 3,000 miles.

In the spring of 1846, the publication relating to Russia was hardly completed, when M. de Verneuil undertook to bridge over this enormous interval. He was anxious to follow out and compare for the two continents the sedimentary strata, from the lowest fossiliferous beds up to the coal. Such was the task laid down for himself by this intrepid pioneer in

science. His study had exclusively for its base the species which he examined in local collections or those which he himself gathered from the deposits. He established the fact that in countries so remote the first appearance of life was very similar in forms, and that the same types were developed successively and in a parallel manner through all the series of Paleozoic beds; that there is on both sides of the ocean a striking agreement in the succession of life.

De Verneuil has then the double credit, on one hand, of carrying to the United States an intimate knowledge of the divisions in the Paleozoic strata established in Europe, and on the other, of bringing back to Europe the results of American investigations, and the possibility of profiting by them: de Verneuil resolved the complex question by his own labors. His modest presentation of the parallelism of the Paleozoic rocks of the two countries has lost nothing of its merit, notwithstanding the constant advance of the science; it is still a fundamental position. This memoir established the place to which paleontology is entitled in investigations relating to the history of the globe. This is perhaps the crowning labor of M. de Verneuil.

There is, however, another enterprise which bears still higher testimony to his boundless devotion to science and his indefatigable perseverance. Spain had been less studied than any other part of Europe, when M. de Verneuil thought of turning his steps thither. He was also drawn to it by Blainville, who did not believe in the universality of the laws of paleontology. If the succession of the beds, and of the fauna characterizing them, seemed to be established over the northern portions of the two continents of Europe and America, this great naturalist supposed that in Spain, in the south principally, the order of succession of fossil species might be reversed, or at least modified; an idea which was far from being realized.

From 1849 to 1862, de Verneuil made not less than a dozen trips into the peninsula, sometimes alone, and sometimes with M. Edouard Collomb, who was already known by his investigations of ancient glaciers. Sometimes also he was accompanied by young naturalists, who went for the sake of his instructions. The geological map of Spain, and the memoirs published after these laborious excursions, among others that which is rendered signal by the discovery of the Primordial fauna, did not interest Spain alone, where they incited others to labor, but the whole scientific world.

It is greatly to be regretted that the author of so many precious observations did not find time to put them in order and work them out as a whole, as he did in Russia.

In 1854, de Verneuil was made member at large of the Academy of Sciences at Paris. The Royal Society of London and other foreign societies had enrolled him among their associates.

His taste for traveling, which was so important in its results to geology, was not lessened when the increasing failure of his sight deprived him of its chief pleasure. The privations he endured in rough or inhospitable countries had never diminished his zeal, or his good humor. Many times he ran great risks, as when he ventured dangerously near a volcanic eruption at Vesuvius or at the island of Santorin.

A clear judgment, and a complete independence of all preconceived ideas, guided him in his deductions. Far from being absorbed in his own special studies, he interested himself in various other branches of human knowledge. He was perfect master of many modern languages; and this was one of the means which insured him success in the countries he explored. Moreover, the fine arts were not excluded from his pleasures; he had cultivated his talent for music even to becoming a skillful composer. Few persons possessed more natural benevolence than he. His extreme kindness was not only apparent in the ordinary intercourse of society, but was manifested in many generous deeds. He discussed the opinions of his opponents with calmness and good temper. Exquisite loyalty and sincere modesty were the dominant traits of his noble character. He found more pleasure in bringing forward the discoveries of another than his own, and, perhaps for that reason, more than one of his associates failed to appreciate the full extent of his merits.

During the illness which for three months interfered with his activity, he continued to interest himself warmly in all the facts of science as well as in very different questions; through all, his good humor never failed. He preserved his serenity even to the last, and died, as a Christian, on the 29th of May, 1873, having almost completed his 68th year.

M. de Verneuil had called to his aid all the resources of paleontology, particularly those pertaining to the Paleozoic strata. In this respect he held perhaps the first rank among geologists on both continents. Moreover, he was at once the pioneer and the master in the knowledge of North American geology.

It was not only by his published works that de Verneuil contributed to the advance of science. He has raised to himself a monument in his collections, in which he brought together types of the choicest fossils selected from the various regions he had explored. Strangers from all countries, as well as French savants, have availed themselves of his valuable collections,

which with the utmost liberality he put at their service, besides aiding them with the results of his own studies. In this way he was a center for the study of paleontology, whence radiated light in every direction.

To continue, even after he had gone, the exercise of his generosity toward all students, he has desired that his collections—certainly unique—should be always at their disposal, and to this end he bequeathed them to the Gallery of the School of Mines.

The memory of our excellent member will always be held in reverence among the geologists and paleontologists of the world.

ART. XXX.—*On Isomeric Sulphosalicylic Acids;* by IRA REMSEN.

IN the hope of finding a simpler method than that at present employed for the preparation of oxysalicylic acid, I turned my attention to the sulpho-acid. The preparation of this acid under the most unfavorable circumstances must be simpler than that of the iodo-acid; and, further, it appeared probable that, if the desired conversion of the SO^2OH group into hydroxyl took place at all, this would be a cleaner change than that which accompanies the action of potassic hydroxide upon iodosalicylic acid.

Sulphosalicylic acid was originally prepared by Cahours,* but was first carefully studied by Mendius.† These chemists prepared it by allowing the vapor of sulphuric anhydride to act upon salicylic acid. On dissolving the product in water, and filtering off from unaltered salicylic acid, an aqueous solution of the sulpho acid was obtained, and from this, by proper treatment, the salts and the free acid.

I found it simpler to treat salicylic acid directly with ordinary concentrated sulphuric acid. Solution took place very readily with the aid of slight heat. The solution was somewhat strongly discolored. On diluting with water and allowing to stand, no unaltered salicylic acid was deposited. The whole was neutralized by means of finely powdered chalk, and then filtered off from excess of chalk and precipitated gypsum. The solution of calcium salts, thus obtained, was treated with potassic carbonate until a precipitate was no longer formed, when the calcic carbonate was filtered off. The filtrate, containing the potassium salts, was evaporated down to a comparatively small volume. On now being allowed to cool slowly, a

* Ann. Ch. Phys., [3] xiii, 92. † Ann. Ch. Pharm., ciii, 39.

deposit of crystals was formed. These were removed and recrystallized. Thus they were obtained in the form of beautiful thick needles, almost colorless. Although subsequently recrystallized a number of times, they retained a slightly yellowish tinge. These crystals, which presented every appearance of a chemically pure substance, were analyzed with the following results:

I. 0·5885 grams of the salt, on being heated to 190°, lost 0·0637 grams H^2O=10·83 per cent.

II. 0·5885 grams of the salt gave 0·3113 grams K^2SO^4=0·13956 grams K=23·71 per cent.

		Calculated.	Found.
$(C^7H^4O^6S)$	216	65·45	----
K^2	78	23·64	23·71
$2H^2O$	36	10·91	10·83
	330	100·00	

The fact that this salt contains two molecules of water of crystallization, together with the fact that it is the principal product of the reaction under consideration, renders it probable that it is identical with the potassium salt prepared and described by Mendius (loc. cit.), as the latter also contains two molecules of water of crystallization.

On evaporating the mother liquor from the crystals first obtained, and again allowing to crystallize, a second deposit took place. This was made up to a great extent of crystals of the same kind as that already described, but, in addition to these, there were noticed others which were larger, more compact and well-formed. The two kinds were separated as carefully as possible by mechanical means, and both recrystallized repeatedly. The larger crystals, after being subjected to this process, were nearly colorless, and presented the form of sections of quadratic (?) pyramids. Some of them attained considerable dimensions; all were well developed; and none possessed the slightest resemblance to the crystals of the first set. They were analyzed with the following results:

I. 0·5258 grams of the salt, on being heated to 180°, lost 0·044 grams H^2O=8·37 per cent.

II. 0·5258 grams of the salt gave 0·2859 grams K^2SO^4=0·12817 grams K=24·38 per cent.

		Calculated.	Found.
$(C^7H^4O^6S)$	216	67·29	----
K^2	78	24·30	24.38
$1\frac{1}{2}H^2O$	27	8·41	8·37
	321	100·00	

The salt is thus proved to be derived from a sulphosalicylic acid; and, further, the difference in form and formula between it and the needly crystals make it appear probable that the two salts are derived from isomeric modifications of the same acid, though, of course, the proof is not given. This supposition is rendered still more probable by the fact that when a substituting agent is allowed to act upon an aromatic body, in almost all cases where it is theoretically possible, two isomeric products are formed; the formation of but one product being, as recent investigations tend to show, a rare exception.

If the ordinary reaction of Dusart, Kekulé and Wurtz could be applied with success to the two salts described, it would be possible to throw some much-needed light upon the constitution of the dioxybenzoic acids, of which we can safely say that the constitution of not a single one of the many varieties described is known. Provided we can once establish the fact that molecular rearrangement does not play an important role in the varied metamorphoses of aromatic compounds; provided, particularly, we can demonstrate that hydroquinone, pyrocatechin and resorcin are not mutually transformable under the influence of heat; and that there is more than a chance connection between the arrangement of the hydroxyl groups in the dioxybenzoic acids and the products which they yield when subjected to dry distillation, then the problem of determining the constitution of these acids becomes a very simple one. A careful consideration of the subject will show that it is only necessary to know in regard to each particular acid: 1st, from which of the oxybenzoic acids it is obtained, and, 2nd, which of the three dioxybenzenes it yields when distilled. From these starting points the conclusions may be easily drawn. There are, according to Kekulé's hypothesis, but six varieties of these dioxyacids possible, most of which are probably known; though they have been prepared in such small quantities, and studied so superficially, that our knowledge of them is exceedingly limited. I hope soon to be able to give this subject further attention. As remarked above, the study of the isomeric sulphosalicylic acids promised reward in the direction indicated. I hence endeavored to convert the sulpho-group of each of them into hydroxyl. Unfortunately, this conversion could not be effected, though, at the same time, another interesting and significant result was attained.

The potassic sulphosalicylate first obtained, viz: that which crystallized in the form of needles, was fused with potassic hydroxide in a silver basin, the mass being constantly stirred with a silver spatula. A change of color accompanied the reaction, resembling that observed in connection with the corresponding reaction between potassic sulphoxybenzoate and

potassic hydroxide. As soon as the mass ceased to foam it was allowed to cool; then dissolved in water, acidulated with sulphuric acid, and agitated with ether. The ethereal extract, on being distilled over a water-bath, left behind a brown, thick, oily residue, decidedly unattractive. This was boiled with water, and thus partially brought into solution, while at the same time a portion of the substance was volatilized with the aqueous vapor, and deposited upon the upper walls of the vessel in the form of very delicate, silken needles. The whole was boiled with a large quantity of water, and the vapors which passed over condensed. The distillate was clear with the exception of a few oily drops which floated on the surface. For a long time the vapors continued to show an acid reaction, but, after boiling for several hours, the process was discontinued. On now again extracting the dissolved substance by means of ether, both from the distillate and the contents of the flask, the products were found to be very similar in appearance, and both resembled the original "brown, thick, oily residue." They were allowed to dry over sulphuric acid, when they became compact and hard. Finally, by carefully subliming these products between watch-glasses, through filtering paper, a pure sublimate was, in both cases, obtained, while, also in both cases, an oily residue was left behind. The sublimate consisted of delicate colorless needles of silken luster. These were found to fuse at 156°. They were readily soluble in hot water, and crystallized out on cooling in long needles. Their aqueous solution when treated with ferric chloride turned a deep violet color. These are the properties of salicylic acid, and an analysis was alone necessary to prove that this acid was indeed the principal product of the reaction:

0·2058 grams of the substance, dried over sulphuric acid, gave 0·4606 grams CO^2=0·12562 grams C; and 0·0882 grams H^2O=0·0098 grams H.

		Calculated.	Found.
C^7	84	60·87	61·04
H^6	6	4·35	4·76
O^3	48	34·78	----
	138	100.00	

The other product formed could not by the most varied manipulations be rendered more attractive. It remained a disagreeable, thick oil. This had the odor and other general properties of phenol. Its aqueous solution, treated with bromine water, gave a yellow precipitate; and this yellow precipitate, treated with sodium-amalgam, gave a substance which possessed the odor of phenol.* As nothing else could be detected, these

* See Landolt, Ber. Berichte, IV Jahrgang, 770.

two, viz.: salicylic acid and phenol, may be considered the legitimate products of the action of potassic hydroxide upon potassic sulphosalicylate.

It remained to study the corresponding reaction with the second variety of potassic sulphosalicylate. The experiment was performed in exactly the same manner as in the first case; and all the phenomena observed were but repetitions of those already described. Salicylic acid and phenol were the only products. The salicylic acid was not analyzed, as its properties were a sufficient guarantee of its identity. The phenol was detected, as in the former case, by means of Landolt's reaction.

If we consider the phenol as a secondary product, formed by the splitting up of salicylic acid under the influence of heat, we thus see that the effect of the action of potassic hydroxide in the cases under consideration is simply to cause a substitution of the SO^2OH group by H. Similar instances are mentioned in chemical literature, though their number is not great enough as yet to permit of any well-founded general conclusion in regard to this kind of action. It remains to be seen whether any connection can be traced between the positions of the substituting groups, and the occurrence of this retrograde substitution.

Williams College, Mass., August, 1873.

ART. XXXI.—*On the Silt Analysis of Soils and Clays;* by EUG. W. HILGARD, State Geologist of Mississippi.

(Read at the Portland Meeting of the Am. Assoc. Adv. Sci., August, 1873.)

AMONG the objections raised against the utility of soil analyses as mostly made and stated heretofore, not the least serious one is that they do not indicate with any reasonable degree of accuracy, or in a generally intelligible manner, those important points in the physical condition of soils which are practically designated as "lightness," "heaviness," "openness," etc. Indeed, the very idea of what constitutes a sandy soil or a clay soil is exceedingly indefinite; necessarily so, so long as the constituent ideas of "clay" and "sand," respectively, remain so ill defined.

It makes a material difference whether the grains of sand contained in a soil or clay are prevalently half a millimeter in diameter, or the tenth or twentieth part of that amount. Sand (or more properly silt) of the latter size is by no means impalpable; and yet a soil containing 50 per cent of this substance might be exceedingly "heavy," while it would be "light" if the sand grains approached 0·5^{mm} diameter. And it would

make an equally material difference whether or not the impalpable matter usually classed as "clay" were really, in the main, silicate of alumina, or simply silex, or other mineral powder.

Equally important are, of course, the corresponding differences in the properties of clays intended for use in the arts.

In the prosecution of my researches on the soils of the State of Mississippi, I found myself confronted by these difficulties, and by the necessity of providing for some mode of operation, and means of designating the several physical constituents of soils, which should not only insure more accurate results, but should also render these capable of ready comparison all the world over.

I need not recapitulate the often-discussed objections to Nöbel's apparatus, with its four vessels of ever-varying capacity and slope of sides, and variable head of pressure. Not one of the five sediments obtainable by its use is ever of a character approaching uniformity; and even in one and the same instrument, successive analyses of one and the same material differ widely in their results.

Schultze's elutriating apparatus, as modified and used by Fresenius in his investigations of the clays of Nassau—a tall, conical champagne glass, with an adjustable stream of water descending through a tube in the axis—answers a better purpose; but offers the inconvenience of the accumulation of heavy sediments around the mouth of the tube, whereby not only the velocity of the stream is changed, but its failure, at low velocities, to agitate the whole mass of the substance under treatment, allows portions of the latter to escape the elutriating action altogether. And since in soil analysis special importance attaches to these finer sediments, which are carried off at low velocities, this objection is a capital one.

Intending to carry out in a convenient form the idea (already urged by Türrschmidt, Notizblatt, v, 180) of substituting for the accidental and indefinite products usually appearing in the statements of silt-analyses, sediments of known and definite "hydraulic value," I adopted in place of a variable head of water, a constant one (a Mariotte's-bottle arrangement, adapted to ten-gallon carboys), modifiable by means of a stopcock* with a long lever moving on a graduated arc, on which the positions corresponding to given velocities in vessels of known cross-section of mouth, are marked off according to empirical determinations.

In order to obviate the inconvenience arising from the accumulation of sediment around the orifice of the tube delivering the current, I introduced an intermediate conical relay reservoir (R, fig. 2; a test glass, cut short) at the point of the

* For figures, see Plate I.

elutriator (inverted) cone. The smallness of the lower orifice of the latter renders the current there sufficiently rapid to prevent any portion of the sediment concerned at a given velocity from falling into the relay; and whatever sediment does accumulate there can at any time be stirred and brought back into the elutriating vessel, by increasing the velocity for a few seconds of time.

Following up with the microscope the character of the sediments so obtained with the apparatus, fig. 2, I soon found that they were throughout of a very mixed nature; and searching for the cause, I found one in the abruptly conical termination of the elutriator, at C, where the efflux tube was at first attached. For in that case, the ascending current does not decrease regularly its velocity as the cone expands, but is broken up into a complicated system of eddies, whose general tendency is to ascend in the axis of the instrument, and descend at its sides. So far, therefore, from corresponding to the calculated velocity belonging to the cross section at C, the sediment carried off represents the variable effects of these eddies.

The obvious remedy was to adapt to the wide (upper) end of the elutriator tube a cylindrical portion, as shown in the diagram, above C. When the length of this cylinder is made not less than 125^{mm}, no perceptible eddies reach the efflux tube; and the sediments exhibited a pretty satisfactory uniformity of grain, save in so far as the coarser ones still contained a good deal of fine material.

However, in subjecting the working of the instrument to the test of the balance, I found the results still quite unsatisfactory, and apparently inconsistent, especially as regards the finer sediments.

The cause of these anomalies became apparent upon attempting to work over, the second time, a quantity of sediment originally obtained at the velocity of 1^{mm} per second. It should all, of course, again have passed over at the same velocity; but to my surprise, barely one-half of it did so, while a heavy, coarse sediment collected in the lower portion of the elutriating tube, and even settled into the relay reservoir R; as roughly shown in fig. 2. On returning the portion that had passed over to the elutriating vessel, the same phenomenon recurred; and by repeated "cohobation," I finally succeeded in getting about four-fifths of the whole quantity of sediment settled into the relay reservoir!

On examination I found this coarse sediment to consist of flocculent aggregates of from a few to as many as 30 fine particles of siliceous silt. When violently shaken, they part company and become diffused, singly, through the liquid, which then presents simply a general turbidity; the particles then

settling down slowly and singly, at the rate corresponding to their individual size or hydraulic value.

The process of formation of these aggregates may be observed by means of a lens, in all its stages; it being the effect of the downward currents always existing on the sides of the conical vessel, as heretofore mentioned. The aggregation progresses slowly at first; but when once five or six particles have thus coalesced, they begin to descend with increased rapidity, and, growing, avalanche-fashion, as they roll down, finally drop through the narrow lower orifice, despite of the rapid current existing there, into the relay reservoir R.

I have vainly attempted to obviate this trouble in various ways. Even when a central core is introduced in the axis of the conical tube, so as to force up the current close to the sides, return currents *will* form, and with them these miniature avalanches.

It is obvious that this circumstance completely vitiates all determinations heretofore made in conical vessels; whether those of Nöbel's apparatus, or those of Schultze and Fresenius; or even the later ones of Müller, and of Schöne;* in all of which the agitation produced by the current is alone employed for stirring.

The tendency to coalescence diminishes, of course, as the size of the grains increases; but does not altogether cease until their diameter exceeds $0{\cdot}2^{mm}$, or about 16^{mm} hydraulic value. For the elutriation of coarser sediments, hydraulic stirring may be successfully employed. For finer sediments, however, the use of *cylindrical* vessels, and of *rapid agitation by outside power*, seems indispensable.

Fig. 1 of the diagram shows, on a somewhat enlarged scale, the instrument I have devised, with this end in view. The cylindrical elutriating tube T, of $34{\cdot}8^{mm}$ inside diameter at its mouth, and 290^{mm} high, has attached to its base a rotary churn P, consisting of a porcelain beaker triply perforated, viz: at the bottom, for connection with the relay reservoir R; and at the sides, for the passage of a horizontal axis A, bearing four grated wings. This axis, of course, passes through stuffing boxes, firmly cemented to the roughened outside of the beaker, and provided with good, thick leather washers, saturated with tallow. These washers, if the axis run true, will bear a million or more of revolutions without material leakage. From five to six hundred revolutions per minute is a proper velocity, which may be imparted by clockwork, or a turbine.

As the whirling agitation caused by the rotation of the dasher would gradually communicate itself to the whole column

* I regret having been unable to obtain, for reference, the original papers of the two latter authors; the most thorough, probably, heretofore published on this subject.

of water, and cause irregularities, a (preferably concave) wire screen of $0{\cdot}8^{mm}$ aperture is cemented to the lower end of the cylinder. No irregular currents are then observed beyond about 75^{mm} above the screen, whose meshes are yet sufficiently wide to allow any heavy particles or aggregates to sink down freely. Any grains too coarse to pass must, however, be previously sifted out.

Thus arranged, the instrument works quite satisfactorily; and by its aid, soils and clays may readily be separated into sediments of any hydraulic value desired. But in order to insure correct and concordant* results, it is necessary to observe some precautions, to wit:

1. The tube of the instrument must be as nearly cylindrical as possible, and must be placed and maintained in a truly vertical position. A very slight deviation from the vertical at once causes the formation of return currents, and hence of molecular aggregates, on the lower side.

2. Sunshine, or the proximity of any other source of heat, must be carefully excluded. The currents formed when the instrument is exposed to sunshine will completely vitiate the results.

3. The Mariotte's bottle should be frequently cleansed, and the water used be as free from foreign matters as possible. For ordinary purposes, it is scarcely necessary to use distilled water; the quantities used are so large as to render it difficult to maintain an adequate supply; and the errors resulting from the use of any water fit for drinking purposes are too slight to be perceptible, so long as no considerable development of the animal and vegetable germs is allowed. Water containing the slimy fibrils of fungoid and moss prothallia, vorticellæ, etc., will not only cause errors by obstructing the stopcock at low velocities; but these organisms will cause a coalescence of sediments that defies any ordinary churning, and completely vitiates the operation.

4. The amount of sediment discharged at any one time must not exceed that producing a moderate turbidity. Whenever the discharge becomes so copious as to render the moving column opaque, the sediments assume a mixed character; coarse grains being, apparently, upborne by the multitude of light ones whose hydraulic value lies considerably below the velocity used; while the churner also fails to resolve the molecular aggregates which must be perpetually re-forming, where contact is so close and frequent.

This difficulty is especially apt to occur when too large a quantity of material has been used for analysis, or when *one* sediment constitutes an unusually large portion of it. In either

* Usually within 5 per cent of the quantities found.

case, a portion of the substance may be allowed to settle into the relay reservoir, until the part afloat in the churn and tube is partly exhausted; after which, the rest can be gradually brought up and worked off. Or, the sediments shown by the microscope to be much mixed, may be worked over a second time. Either mode, however, involves so grievous a loss of time, as to render it by far preferable to so regulate the amount employed, that even the most copious sediments can be worked off at once. Within certain limits, the smaller the quantity employed, the more concordant are the results. Between ten and fifteen grams is the proper amount for an instrument of the dimensions given above.

I have found that, practically, 0·25^{mm} per second is about the lowest velocity available within reasonable limits of time; and that by successively doubling the velocities, up to 64^{mm}, a desirable ascending series of sediments is obtained; provided always, that a proper previous preparation had been given to the soil or clay.

Preliminary Preparation.—As regards this point, which is of capital importance, I premise that I find the usual precept of boiling from 30 to 60 minutes almost absurdly inadequate to perform that loosening of the adherence of particles, which is the fundamental condition of success in any process of mechanical separation. In no case have I found less than six hours incessant and lively boiling even approximately sufficient; and even with double that time, so much of the disintegration is often left to be done by the churner of the instrument, as to protract indefinitely the exhaustion of the finer sediments, which are then continually being set free from the coarser portions. Thus, in average cases the sediment of 0·25^{mm} h. v. may be "run off" in the course of 30 to 35 hours. But in one case, after 12 hours boiling, the 0·25 sediment gave no sign of disappearance after 36 hours, and continued to come off for 54 hours more, with the coarser sediments.

It is therefore a material saving of time, and essentially promotive of accuracy, to effect the mechanical disintegration in the most thorough manner, beforehand. This can rarely be done without long protracted boiling, and the subsequent use of mechanical means (kneading) on the finest sediments. But I cannot see the propriety of using chemical solvents for disintegration, unless the investigation is to extend beyond the physical properties of the substance treated. The miniature Loess puppets, consisting of sand-grains cemented by carbonate of lime; the grains of bog ore, or alumino-siliceous aggregates found in some soils, fulfill, physically, the same office as solid sand-grains of corresponding size; and should appear as such in the analytical statement.

The presence of clay in the instrument would materially interfere with the proper separation of sediments. In consequence of its property of indefinitely fine diffusion in water, clay—i. e., the hydrous silicate of alumina—produces the same effect as would the dissolution of a salt, viz: increases the buoyant effect, and therefore the hydraulic efficacy of water, to such an extent as to enable it to carry off, e. g., sediment pertaining to the velocity of 1^{mm} in pure water, when the actual velocity is but $0{\cdot}25^{mm}$.

In view of these facts, I have adopted the following course of preliminary treatment:

1. Boiling livelily, for 24 to 30 hours, 15 to 20 grams of weighed "fine earth."

This is best done in a thin, long-necked flask of about 1 liter capacity, filled $\frac{4}{5}$ full of distilled water, and laid on a stand at an angle of 40–45°. It is provided with a cork and condensing tube of sufficient length (5–6 feet) to condense all or most of the steam formed when lively ebullition is kept up by means of a gas flame. For the first few hours, the boiling generally proceeds quietly; but as the disintegration progresses, violent bumping sets in, which sometimes endangers the flask, but is of material assistance for the attainment of the object in view. In extreme cases, some of the heavier sediment (generally clean sand) may be removed from the flask; but this is undesirable. It is frequently the case that when the boiled contents are left to settle, the liquid appears perfectly clear within an hour; although so soon as they are largely diluted, the clay becomes diffused as usual, and will not settle in weeks. Probably this is owing to the extraction from the soil of soluble salts, which exert the same influence as does lime or common salt, even in very dilute solutions.

2. The boiled fluid and sediment is transferred to a beaker, and diluted so as to form from 1 to $1\frac{1}{2}$ liters in bulk; and being stirred up, is allowed to settle for such a length of time as (taking into account the height of the column) will allow all sediment of $0{\cdot}25^{mm}$ hydraulic value to subside; the process being repeated with smaller quantities of fresh water, until no sensible turbidity remains after allowing due time for subsidence.

It must be remembered that this time is considerably longer than that for pure water, so long as any considerable amount of clay remains in the liquid, rendering it specifically heavier. And as the precise amount of allowance to be made cannot in general be foreseen, some sediment of, and exceeding, $0{\cdot}25^{mm}$ h. v. will almost inevitably be decanted with the successive clay waters, until the buoyant effect of the clay becomes insensible. The united clay waters (of which there will be from four to

eight liters) must therefore be again stirred up, and the proper time allowed for the sediments of 0·25^{mm}, and over, to subside. The dilution being very great, a pretty accurate separation is thus accomplished; the sediments being then ready for the elutriator.

Treatment of the "Clay Water."—I have based on the well-known property of clay, of remaining suspended in pure water for weeks and even months, an obvious method of separation from at least the greater portion of silts finer than 0·25^{mm} hydraulic value ($<0·25$).

The clay water is placed for subsidence in a cylindrical vessel (in which it may conveniently occupy 200^{mm} in height), and is there allowed to settle for at least 24 hours. This interval of time was at first chosen arbitrarily; but I subsequently found it to be about the average time required by the finest siliceous silt usually present in soils, to sink through 200^{mm} of pure water. So long as any sensible amount of clay is present, the time of course is longer, say from 40 to 60 hours, or even more, if the clay be abundant and the liquid not very dilute. The sharp line of separation between the dark silt-cloud below and the translucent clay water above, is readily observed, and the time of subsidence regulated accordingly. At times, several such lines of division may be seen simultaneously in the column, indicating silt of successive sizes, with a break between. No such appearance is presented when, after weeks of quiet, the clay itself gradually settles. The liquid, which may be almost clear at the surface, then shades off downward very gradually, until, near the bottom of the vessel, it becomes entirely opaque.

After decantation of the clay water, the remaining liquid is poured off temporarily, leaving the sediment as dry as possible. It is then rubbed or kneaded in the decanting vessel itself, with long handled rubber pestle (conveniently cut out of a car spring).

Water is then again poured on (agitating as much as possible, to break up the molecular aggregates) to the proper height, and another 24 hours subsidence allowed. This operation is repeated (6 to 9 times), until either the water remains almost clear after the last subsidence, or the decanted turbid water fails to be precipitated by salt water.

It thus seems possible, by a large number of successive decantations, to separate pretty sharply the clay proper from the fine silts. But the amount of time and care required in the process of complete separation is so great, and the difference of percentage resulting from a neglect of the subsidence beyond 24 hours is in most cases so slight, that in the analyses made thus far, I have throughout adhered to the 24 hours interval;

the "clay" thus obtained being, of course, more or less contaminated with some of the finest silt; which is precipitated with it by salt, provided the relative amount of clay is not too small. Otherwise a slight turbidity may remain for several days in the decanted liquid, which cannot then be cleared by the further addition of salt.

50^{ccm} of a saturated brine (i. e., 1·5 per cent of salt) is ordinarily sufficient to precipitate one liter of clay water; the precipitation is much favored by warming. Half the quantity, or even less, will do the same, but more time is required, and the precipitate is more voluminous.

As it cannot ordinarily be washed with pure water, it must be collected on a weighed filter, washed with weak brine, dried at 100° and weighed. It is then again placed in a funnel and washed with a weak solution of sal ammoniac, until all the chloride of sodium is removed. The filtrate is evaporated, the residue ignited and weighed: its weight, *plus* that of the filter, deducted from the total weight, gives that of the clay itself.

In some cases, especially of clays and subsoils deeply tinged with iron, the clay, *after drying at* 100°, will not readily diffuse in water, and can be washed with pure water until free from salt; it can then of course be weighed directly.

[To be continued.]

ART. XXXII.—*Discovery of a new Planet, and observations on that discovered June 13th;* by JAMES C. WATSON.

ON the 29th of July I discovered a planet of the 12th magnitude and observed the following place:

Ann Arbor M. T.	α	δ
1873 July 29 $14^h\ 1^m\ 0^s$	$23^h\ 8^m\ 12^s{\cdot}35$	$-2°\ 23'\ 11''{\cdot}0$.

On account of cloudy weather and bright moonlight I did not have another opportunity to observe it until August 16th. I then found what I supposed to be the planet sought, and I observed it as follows:

Ann Arbor M. T.	α	δ
1873 August 16 $12^h\ 38^m\ 4^s$	$23^h\ 2\ 41{\cdot}50$	$-2°\ 37'\ 59''{\cdot}2$
16 13 14 24	23 2 40·23	2 38 9·5
16 14 2 4	23 2 38·75	2 38 9·2
16 14 59 27	23 2 37·35	2 38 12·8
17 10 25 3	23 2 2·69	2 39 36·7
17 10 46 20	23 2 1·93	2 39 47·1
23 11 45 7	22 57 28·37	−2 52 51·3

By means of these observations I computed the following circular elements:

Epoch=1873 Aug. 23·5 Washington M. T.

$u = 20°\ 5'\ 50''.9$
$\Omega = 319\ 55\ 37.2$ } Apparent Equinox August 23.
$i = 6\ 45\ 5.0$ }
$\log a = 0.467016$ $\mu = 707''.103$

These elements represent well the places of the planet to September 2d, and for July 29th they give the following place:

$\alpha = 23^h\ 12^m\ 41^s.2$ $\delta = -2°\ 23'\ 42''$

It is evident, therefore, that the planet of August 16th is different from that of July 29th. I have commenced the search for the latter, and will prosecute it as completely as the weather will permit. The planet of August 16th is at present about a magnitude brighter than that of July 29th.

I add also some observations of the planet discovered by me on June 13th.

Ann Arbor M. T.				α	δ
1873 June	13	$12^h\ 43^m\ 44^s$		$17^h\ 16\ 59^s.50$	$-21°\ 52'\ 17''.4$
	13	13 37 44		17 16 57.04	21 51 52.9
	14	10 17 9		17 15 56.10	21 42 40.7
	15	11 47 20		17 14 41.68	21 31 29.6
	18	11 51 51		17 11 16.97	20 59 58.1
July	2	13 5 0		16 57 50.4	18 44 50.
	5	13 22 17		16 55 38.43	−18 19 12.7

This planet was of the eleventh magnitude. The following elements have been computed by Mr. Ritter, one of my assistants:

Epoch=1873 June 18·5 Washington M. T.

$M = 79°\ 38'\ 5''.9$
$\mu = 152\ 38\ 14.6$
$\Omega = 259\ 33\ 6.2$ } Ecliptic and Mean Eq. 1873·0
$i = 21\ 7\ 39.7$
$\varphi = 15\ 28\ 27.8$
$\log a = 0.372518$ $\mu = 980''.008$.

Ann Arbor, Sept. 3, 1873.

ART. XXXIII.—*Hayden and Gardner's Survey of the Territories, under the Direction of the Department of the Interior.**

THE U. S. Geological and Geographical Survey of the Territories was authorized by the last Congress to make a careful geological and topographical survey of Colorado Territory. Field work was begun about the middle of May. The region selected for this season's operations lies between parallels 38°

* Communicated by Jas. T. Gardner, Geographer of the Expedition, under date of Middle Park, Sept. 3.

and 40° 20′ north, and between meridians 104° 30′ and 107° west, and contains about 20,000 square miles. It was divided by east and west lines into three districts, each about 58 miles broad by 130 long. The survey of each district was entrusted to a mixed party, of geologists and topographers. The northern district included the Middle Park, the middle district the South Park, and the southern the San Luis Park. Each district presents a complete section of the main upheavals of the Rocky Mts. The whole area surveyed may be regarded as a section, 160 miles broad, of the grandest ranges of the Rocky Mountain system at their point of greatest elevation; and as the most extended region of high peaks to be found in the country. From Mt. Lincoln, which is nearly in the center, we counted 200 peaks of 13,000 feet or over.

Between latitude 38° 30′ and 40° 30′, we find the Rocky Mts. composed of three great parallel meridional ranges; and west of these a grand group, of complex structure, called the Elk Mts., and high plateau-like ridges, sloping westward.

To the most eastern of the three ranges we give the name of Front Range, since it is the grand front which the Rocky Mts. present to the great plains. Standing on a high roll of the prairie back of Denver, one sees the mountains rise abruptly out of the plains and tower to snowy crests that stretch north and south in a magnificent panorama for 120 miles, from Pike's Peak on the south to a group 20 miles north of Long's Peak. Six of the summits—namely, Long's Peak, Mt. Torrey, Gray's Peak, Mt. Rosa, Mt. Evans, and Pike's Peak—attain an elevation of 14,000 to 14,200 feet above the sea. Our artist, Mr. Holmes, has made a very accurate detailed drawing of the panorama, which we expect to publish, with names, heights, etc.

West of the Front Range lie the great parks, separated from each other by comparatively low or broken cross ridges. Parallel with it, and about 40 miles farther west, is a grand line of mountains, forming the western boundary of the South, Middle, and North Parks. We name this the Park Range. Its highest points are near the junction of the cross range that divides South from Middle Park. They are the Mt. Lincoln group, which includes twenty peaks that exceed 13,000 feet in height; and its culminating points, Lincoln and Quandary, rise above 14,000. Twenty miles farther north, the range becomes a very sharply crested wall, with many 13,000-foot peaks, and with its culminating points rising to 13,300 feet. This we call the Blue River group. To its northernmost and highest summit belongs the name of Mt. Powell, from the gallant explorer by whom it was first ascended in 1868. From Mt. Powell northward there are no high peaks until opposite the North Park, where the range rises again into summits of 12,000 feet and over.

West of the southern part of the Park Range is the Arkansas Valley; and beyond this valley is another grand line of heights, heretofore unnamed, and which we call the National Range. It axis is parallel with that of the Park Range, and only about sixteen miles west of it.. It ends abruptly, about 40 miles northwest of Mt. Lincoln, in a very impressive peak, about 13,400 feet high, called the Mount of the Holy Cross. This steep-sided cone with rounded top fronts the east in a dark precipice 3000 feet high, in the center of which is a brilliant white cross 1500 feet long. The form is quite perfect, and plainly visible to the naked eye at 50 miles' distance. We first saw it from Mt. Evans, and then from Gray's Peak, in the Front Range. After tremendous labor,* Mr. Jackson succeeded in obtaining admirable photographic views of the precipice and cross, from a distance not exceeding a mile.

The highest part of the National Range commences about 20 miles south of the Holy Cross, in Grand Mountain, opposite the town of Oro, in the Arkansas Valley. Grand Mt. is about 14,200 feet high, and from here to 50 miles farther south the whole range is elevated to 13,000 feet, while there are ten peaks that rise above 14,000 feet, some of them doubtless reaching 14,400. These culminating points rise at intervals of five to eight miles along the crest of the range. The one next south of Grand Mt. is Mt. Elbert; the next is La Plata Mt.; then come the peaks named by Prof. J. D. Whitney in 1869 Mt. Harvard and Mt. Yale, and so on.

The National Range is one of the grandest on the continent. Through nearly its whole extent it forms the divide between the waters of the Atlantic and Pacific Oceans.

West of this range, and connected with it, is a great group of mountains, lying in the triangle between the Grand River on the north and the Gunnison River on the south, and known as the Elk Mts. Owing to the complexity and variety of their structure, they are of the highest imaginable interest to the geologist. The part that eruptive granite can play in mountain-building is here completely demonstrated. The most elevated peaks of this group form a ridge about 30 miles long, nearly parallel with the National Range, and some 35 miles west of it. At the northern end of this line of elevation, in lat. 39° 15′ N., is Sopris Mt., a long dome-shaped ridge, about 13,000 feet high. Ten miles to the south of it is a sharp cone, overtopping all its neighbors, and rising to about 14,100 feet; this we have named the Capitol. Three miles further south is another great peak, only about 50 feet lower, which we call the

* Mr. Jackson, Mr. Coulter the botanist, and one packer, carried 100 lbs. of photographic apparatus for ten hours over rocks and fallen timber and up 4000 feet of steepest débris slope.

White House, from the conspicuous snow-field, about a mile in horizontal breadth and having a slope of half a mile, which covers its eastern front. This snow-mass is by far the largest we have found in the mountains of Colorado, and distinctly marks and characterizes the peak, even as seen from the Front Range, 80 miles away. Yet further south, five and ten miles respectively, are two 14,000-foot peaks, of dark red sandstone, which we call Maroon Mt. and Castle Peak.

West of this group there are no high mountains; within 20 miles the ridges change into plateaus, which fall off to the Colorado River.

We expect to publish accurate panoramic drawings of each of these four ranges; nothing but these and photographs can give an idea of their grand forms and interesting structure.

It seems very remarkable that, in a region where there are so many high peaks, the culminating points should all range between 14,000 and 14,500 feet. So far as I now know, there are 22 summits of this class in our district.

The primary and secondary triangulation, on which all the work is based, has been successfully carried through, in spite of the difficulties that attend the carrying of large instruments to the top of such lofty peaks. The 8-inch theodolite and tripod, weighing about 50 lbs., have sometimes to be carried on the back up ascents of 3,000 to 5,000 feet, over the steepest rocks. By October 1st, the primary triangulation will have been extended over 30,000 square miles.

ART. XXXIV.—*New Observations on the Dinocerata;* by O. C. MARSH.

MANY additional remains of the Dinocerata have been obtained by the Yale College expedition during the past summer, and many doubtful points cleared up in regard to the structure of these animals.

1st. The dental formula of this order, so far as now known, is as follows:—incisors $\frac{0}{3}$, canines $\frac{1}{1}$, molars $\frac{6}{6}$, $\times 2 = 34$.

2d. The premaxillaries are not united in front, and vary much in form in different species.

3d. The lower jaw has no true proboscidean features, but resembles that of the *Hippopotamus*, especially in the great downward extension of the rami below the diastema.

4th. It is extremely probable that both sexes were provided with horns.

5th. It is possible that the osseous protuberances on the skull did not all support true horns.

6th. There were five toes in the manus, with metacarpals moderately elongated.

7th. There were but four toes in the pes, with short metatarsals.

8th. The characters of the order include marked perissodactyl, artiodactyl, and proboscidean features, the last being apparently the least developed.

9th. The geological horizon of all the known animals of this group is Upper Eocene.

A new and well marked species of *Dinoceras* is represented by a nearly perfect skull with the lower jaw entire, and by various other parts of the skeleton. It differs especially from *D. mirabilis* Marsh, the type species, in the greater proportionate width of the skull, the shorter and more massive posterior horn-cores, and the more compressed and prominent nasal cones. The species, moreover, was of greater size. The entire length of the skull was 33·0 inches. The width between outer faces of the occipital condyles was 7·8 inches, and across the zygomatic arches 13·5 inches. The lower jaw measured 11·6 inches between the outer ends of the condyles, and its length from condyle to front of symphysis was 20·8 inches. The canine of the lower jaw was smaller than the last incisor, and slightly separated from it. The three incisors decreased in size back from the symphysis, and all were directed well forward. This species is from the Upper Eocene of Wyoming, and may be called *Dinoceras laticeps.*

Fort Bridger, Sept. 1, 1873.

SCIENTIFIC INTELLIGENCE.

I. Physics and Chemistry.

1. *On the Measure of Work in the Theory of Energy.* By Robert Moon, M.A., Honorary Fellow of Queen's College, Cambridge.—Professor Maxwell gives the following definition and measure of work: "Work is done when resistance is overcome; and the quantity of work done is measured by the product of the resisting force and the distance throughout which that force is overcome." (Theory of Heat, 1871, p. 87.)

(1.) It is to be presumed that when the uniform force F acts throughout the time T in a given direction upon a body which is free to move in that direction, the resistance overcome by the force will be that arising from the *inertia* of the body—in other words, the resistance which the body offers to any change being effected

in its state of rest or motion for the time being, and which is always proportional to the force employed in overcoming it. It follows from this, that, under the circumstances referred to, the resistance which is being overcome at each epoch of time, and therefore the work done in equal intervals of time, will be the same throughout the motion. But if the body is at rest to start with, and T is divided into n equal intervals, the work done at the end of the first interval $\frac{T}{n}$, according to the above measure, will be $\frac{1}{2} F^2 \frac{T^2}{n^2}$; that done at the end of the second interval will be $\frac{1}{2} F^2 \left(\frac{2T}{n}\right)^2$; that at the end of the third interval will be $\frac{1}{2} F^2 \left(\frac{3T}{n}\right)^2$, etc.: and therefore the work done in the first interval $\frac{T}{n}$ will be $\frac{1}{2} F^2 \frac{T^2}{n^2}$; that *during* the second interval will be $\frac{3}{2} F^2 \frac{T^2}{n^2}$; that *during* the third interval will be $\frac{5}{2} F^2 \frac{T^2}{n^2}$, etc. It has already been pointed out, however, that the foregoing definition of work implies that the amount of work done in equal intervals of time will be the same. It follows, therefore, that the definition and measure of work above propounded contradict each other in the case we have been considering.

(2.) Suppose a body whose mass is M to be moving in a certain direction with a velocity V_1, and that the force F is applied to the body in the direction of its motion. Professor Maxwell proves that, if during the small time T the body moves through the space s, and has acquired at the end of T the velocity V, we shall have

$$Fs = \tfrac{1}{2} (MV^2 - MV_1^2),$$

an equation, be it remembered, which holds independently of the magnitude of T, provided F be uniform.

If we put $V = V_1 + v$, we shall have

$$\text{work} = Fs = \tfrac{1}{2} M (v^2 + 2vV_1) \quad . \quad . \quad . \quad (1)$$

Now v is the pure product of the force F acting on the body M during the time T; whence it appears that, adopting the measure of work above proposed, the work done by the force F on the body M in the time T involves the variable quantity V_1, which is entirely independent alike of F, of M, and of T.

(3.) The right side of the expression (1) will always be positive so long as V_1 and v have the same sign, i. e., so long as the directions of the force and the initial velocity conspire. But if the force and initial velocity have opposite directions, and T and V_1 are both finite, the right-hand side of (1) will first be negative; as the motion proceeds it will become zero; and it will finally become and continue positive. It results, therefore, from the above measure of work, that the work done in a finite time by a

finite force acting upon a body of finite magnitude which is free to move, may be zero.

(4.) The proper work of force is to generate or destroy momentum;* and the work done by the force in a given time will be properly measured by the momentum created or destroyed in that time.†

The measure thus proposed, in fact, differs from the received measure enunciated by Professor Maxwell less than might at first sight be supposed; for when T is very small, (1) becomes

$$\text{work} = Fs = Mv.V_1.$$

Thus, while I contend that the work done in a short interval of time under the above circumstances is properly measured by the momentum generated during the time, according to the views upon the subject which are generally received, it is measured by the product of the momentum generated and the initial velocity V_1—a position the reasonableness of which, I apprehend, it will be found difficult to establish.

(5.) If, instead of expressing the work done in terms of the force acting and the time during which it acts, we wish to express it in terms of the force and of the space described under its influence, we have only to replace T in the expression FT by its equivalent in terms of the other variables. This, where the body moves from rest, would give us $\sqrt{2MFs}$ as the measure of the work done by the force F on the body M while moving through the space *s*.—*Phil. Mag.*, Sept., 1873, p. 219.

2. *Sull' Ozono; Noti e Riflessioni, di* Giuseppe Bellucci, Dottore in Storia Naturale, Professore nel R. Istituto Industriale e Professionale di Terni. Prato, Tipografia Giachetti, Figlio e C. 1869, pp. 456, 8vo.—In this volume, Signor Bellucci, who is now Professor in the University of Perugia, gives a very interesting and valuable *résumé* of the researches upon the subject of ozone, which have occupied the attention of chemists for so many years. The author has drawn his material from the original contributions of the numerous investigators, the sources being carefully indicated by foot-notes. The discussion is divided into five parts, a summary of which will give an idea of the scope and general character of the work.

Part first traces the progress of discovery *in re* ozone from the time of Van Marum, who in 1783 observed the peculiar odor communicated to oxygen by the passage of the electrical dis-

* No doubt force has another effect—that, namely, of causing a body to describe, or of preventing its describing, space: but of these two effects, viz., the description of space and the generation of momentum in any indefinitely small interval of time, the former will be of a lower order of magnitude than the latter; while, of the small space actually described in the interval, all but an indefinitely small portion will have been described under the influence of the velocity from time to time generated during the interval; the residuum immediately due to the direct action of the force, and in no degree resulting from acquired velocity, being of the *third* order of small quantities at most.

† From this it follows that the work done by the force F acting during the time T on a body which is free to move will be measured by FT.

charge, and a year or two later noticed that simultaneously with this effect a diminution of the volume took place, but without having an idea of the significance of these facts, to those of modern chemists, who have so thoroughly cleared up the obscurities which for a long time surrounded this difficult subject.

In part second the author treats of the physical and chemical properties of ozone, and its action upon the animal economy.

In part third are discussed the various modes of producing ozone, and its occurrence in nature.

Part fourth is an exposition of the theoretical views of various chemists relating to the so-called antozone. This is one of the most valuable portions of the book, and it is interesting to see, that from a thorough discussion of the subject, the author concludes that the experiments and theories which were supposed by many to confirm the assumption of an allotropic condition of oxygen antagonistic to ozone are unsound, and that the supposed antozone has in fact no existence. It must be noticed too that the book was published before the more recent researches, especially those of Engler and Nasse, had conclusively proved that the reactions formerly attributed to antozone were really due, in most cases, to the presence of hydrogen peroxide.

In part fifth, he discusses, by comparison, the properties of ozone, of ordinary oxygen, and of oxygen in the nascent or atomic state, with interesting theoretical considerations suggested by the various facts which have been observed.

Although bearing the modest title of "Notes and Reflections," the work is really a full and satisfactory exposition of the theoretical part of the subject, and places under obligation to the author a large class of readers who are interested in ozone and its uses.

Prof. Bellucci has also published more recently a *brochure* upon the subject of the "Hypochlorites as a new Source of Ozone" (Florence, 1873), and another upon "The supposed Emission of Ozone from Plants" (Palermo, 1873), in which, from the fact that his experiments gave negative results, he concludes that the oxygen emitted by plants does not contain an appreciable proportion of ozone. A. W. W.

II. Geology and Natural History.

1. *Mountain Making.*—On page 172 the principle that the bottom of a geosynclinal becomes weakened, as subsidence and surface accumulation go forward, through the rise of the isogeotherms, and that "this in an important degree has made possible the catastrophe in which synclinoria have resulted," is attributed to Prof. LeConte. I should have credited to Prof. T. Sterry Hunt the idea of the weakening of the bottom of a geosynclinal in the manner stated. To this idea, Prof. LeConte added the view that through such a weakening, lateral pressure from the earth's contraction (a force not appealed to in Prof. Hunt's hypothesis) was enabled to produce the catastrophe referred to, and this is the important principle adopted from his memoir. J. D. D.

2. *Cretaceous of Long Island.*—In a notice of the United States geological map of Hitchcock and Blake, on page 64 of this volume, the representation of Long Island as Cretaceous is objected to. Prof. Hitchcock writes us that the authors were guided in their decision on this point, not merely by the report of Prof. Mather on the subject, but also, by more recent and satisfactory observations. Prof. Mather, in his New York Geological Report, bases his conclusion on the existence of a bed containing lignite beneath the drift, and on the fact that shells and some bones had been obtained at various times in digging wells and other ways, although he was unable to find any specimens of the shells or bones in the hands of those who reported such discoveries and knew nothing whatever of the species. Three pages of the report are occupied with statements of facts of discoveries having this indefinite geological value. The evidence was so incomplete that the Cretaceous of Long Island does not appear on the geological maps of either Prof. James Hall or Sir William Logan. We understand that there are recent discoveries which will place Prof. Mather's conclusion on a better foundation. J. D. D.

3. *On the Copper Deposits of the Blue Ridge.*—In my last letter I gave some account of the Ore Knob Copper Mine, in Ashe County, North Carolina, where I then was. I have since examined the copper deposits of Polk County, Tennessee, and Carroll County, Virginia. The first of these are the well-known Ducktown mines, which were well described in the report of Messrs. Trippel and Credner to the American Bureau of Mines in 1866. In this, as in the earlier published reports of Messrs. Whitney, Blake, and others, these deposits of Ducktown are indicated as conforming in dip and strike with the enclosing mica-schist, and not as fissure-lodes. Notwithstanding their apparent intercalation, I am, however, disposed to regard them not as comtemporaneous with the strata, but as subsequent deposits in rifts or fissures. This relation of contemporaneous or posterior deposition is evidently the fundamental distinction in mineral deposits; and in the latter case it is but a secondary consideration whether the opening in the strata, in which the endogenous mineral mass has been formed, is transverse or for greater or less distances conformable to the stratification. It is doubtless in some cases difficult to distinguish between the contemporaneous impregnation of a sediment, as in certain foreign copper-slates and some analogous deposits in various crystalline schists, and the penetration by diffusion, which often attends the subsequent deposition of a metallic ore in fissures, since in the cases where these conform to the strata, the local impregnations sometimes give the aspect of a passage from the bedded lode to the adjacent rock. Fine samples of lodes, intercalated for considerable distances with the strata, are seen in the granite veinstones which abound in the gneisses and mica-schists of the Montalban or White Mountain series in some parts of Maine, as described by me in the *American Journal of Science* for March, 1871; and also in the thin parallel veins which, at

Winslow, in that State, carry cassiterite and mispickel in a gangue of fluor, quartz and mica, and are intercalated between the beds of a micaceous limestone with great regularity, and only here and there are seen to traverse the beds, thus revealing their posterior origin. These were described and exhibited by me at the meeting of the Institute of Mining Engineers in February last. In both of these cases, however, the endogenous character and posterior origin of these deposits is evident from their structure and mineral composition, and it is in like manner that, after a careful study of the Ducktown copper deposits, and a comparison of them with others in Virginia and North Carolina, I feel constrained to regard them as having been formed in the midst of the strata, and as apt, in any part of their distribution, to appear as transverse veins. There are, it is true, in their immediate vicinity, impregnations in the mica-schists which simulate closely those of contemporaneous origin, but the great masses of pyritous ores and associated minerals, as disclosed in the deep workings at the East Tennessee Mine at Ducktown, have all the characters of true veinstones. The massive pyrrhotine and chalcopyrite which form the metalliferous portions of the deposit are traversed by large crystals of zoisite, hornblende and pyroxene, the latter sometimes an inch in diameter and six inches in length. The hornblende crystals are often curved, and sometimes partially broken across, and their transverse fissures filled with sulphurets, which are also occasionally interposed between the cleavage planes of the augite crystals. These silicates are sometimes incrusted with chalcopyrite, with galena, with blende, and more rarely with crystallized pyrrhotine; molybdenite is also met with. But for these associations it would be impossible to distinguish specimens of these silicates from those met with in the great Laurentian veinstones so well known to mineralogists, and the resemblance to the latter is still more complete in the great masses of coarsely cleavable white augite rock which form portions of the veinstone, and in others which are an aggregate of long prisms of greenish hornblende imbedded in a gangue of white crystalline calcite. With these are associated varieties of finely fibrous hornblende, greenish, brownish and snow-white in color. The disposition of these is more or less regular, and in some cases, at least, parallel to the plane of the lode. In one instance a layer of prisms of white hornblende, two or three inches in length, is arranged in a columnar manner transverse to the wall, the interstices being filled with chalcopyrite. Layers of vitreous translucent quartz, with enclosed masses of sulphids, are also found, having all the aspects of ordinary quartzose veinstones. A pale cinnamon-colored garnet, sometimes in crystals half an inch in diameter, is also met with imbedded in quartz, or more frequently in chalcopyrite. The latter also sometimes includes large and fine crystals of mispickel. A collection of the various minerals and mineral aggregates from the present workings at the East Tennessee Mine would, I think, convince the student familiar with veinstones that this great de-

posit, whatever its attitude in regard to the enclosing rocks, is of posterior formation and identical in the mode of its formation with ordinary concretionary veins. Similar evidences, though less completely displayed, are seen at the Isabella and Mary Mines, which are opened on lodes distinct from the East Tennessee Mine.

It is worthy of remark that the garnet here met with is identical in color and in aspect with that found associated with the copper ores of the Ore Knob Mine in Ashe County, North Carolina; and that, passing thence into Carroll County, Virginia, the interstratified copper lodes there found, which are apparently identical with those of Ducktown, are often bounded by layers of vitreous quartz, which sometimes forms large crystals, rounded on the angles and incrusted with metallic sulphids. Similar specimens occur in the clearly transverse lode of Ore Knob. In the Carroll County deposits, fragments of the mica-schist which forms the enclosing rock are found imbedded in the sulphids of the lode, and the contact of the wall-rock with the layer of quartz is perfectly well defined. It should be said that both here and at Ducktown the country rock, like that at Ore Knob, presents the characters of the White Mountain series. Mica-schists, occasionally with garnet, staurolite and kyanite, predominate, and are associated with a blackish or blue-black hornblendic gneiss, and a fine-grained grayish-white gneiss, which at depths where decomposition has not penetrated, resembles the beautiful granitoid gneisses, so much used in Maine and New Hampshire. A stone of this kind is quarried near Hillsville, in Carroll County.

The great ore deposits of Ducktown are certainly very variable in breadth, ranging from 300 feet to not more than twenty. They are, according to Credner and Trippel, occasionally pinched out in their longitudinal extension, and so succeeded by others as to allow of their being described as lenticular masses, arranged *en échélon*. At the same time the length of these is very considerable; one at Ducktown has been worked continuously for black ore for nearly a mile, and another has been opened for a still greater distance in Carroll County, Va.

The curious phenomenon of the black ores of these deposits has often been described, and there seems no reason to question the received explanation that they owe their origin to a reduction, by some as yet unexplained process, of the sulphates formerly generated by oxidation in the upper portion of the lodes, which, as is well known, is changed into a porous mass of hydrous peroxide of iron holding more or less oxides and green carbonate of copper in its lower portions. The analyses by Trippel and others have shown that this black ore is chiefly a sulphid, in which iron sometimes predominates, while more generally it contains a large proportion of copper, and approaches in composition to copper-glance, crystals of which species, according to Mr. August Raht, occur in druses in some varieties of this ore, which, in other cases, approaches more nearly to erubescite or calcopyrite in its composition. Some masses of these ores hold grains of native copper or

crystals of red oxide, while copper-vitriol, as a result of oxidation, often impregnates these more or less porous or cellular ores, the drainage-waters from which yield considerable quantities of the salt. The product of cement copper obtained at Ducktown by passing the water from certain of the workings over scrap iron is equal to about 5,000 pounds monthly, and the waters hold, in some cases, as much as one part in 1,000 of copper, though generally more dilute.

The study of these deposits in Ducktown and in Virginia has convinced me that the rich purple, gray and black sulphurets of the Ore Knob Mine, described in my last letter, are of secondary origin, though originating in the alteration of a lode of yellow copper ore of phenomenal richness.

These so-called black ores are found in direct contact with the unchanged sulphids of the Ducktown lodes, and it is by an error that the constant existence of a zone of pyrites free from copper, between the overlying black ores and the productive masses of yellow copper beneath, has been asserted. The fact is that these great pyritous lodes vary in composition in different parts, both laterally and vertically; and it has sometimes happened that a portion comparatively poor in copper has been met with just below the black ores, while elsewhere, as at the Mary Mine in Ducktown, excellent yellow ores are found in that position. The breadth of these deposits is very great; the chief western lode at Ducktown varies from twenty to ninety feet, while the great middle lode, on which the Isabella and Eureka Mines are situated, attains more than 300 feet. All portions of these immense lodes not being equally rich in copper, it has been found advantageous, for the purpose of exploring them and of determining what parts may be mined with the greatest profit, to make use of the annular diamond-drill which has just been employed at Ducktown with remarkable success. The great 300 foot lode, dipping to the southeast at a high angle, has been traversed by two borings nearly at right angles to the plane of the lode, and the inspection of the ores removed shows the existence of two large bands of workable copper ore interposed in the enormous mass of pyrites, which require only to be opened by a shaft to greatly augment the produce of this region. Some details of the mining and smelting of these ores, together with observations on the future importance of these deposits as sources both of copper and sulphur, and on the not less valuable and more accessible lodes of a similar kind in Southwestern Virginia, must be reserved for a third letter.—*Engineering and Mining Journal, N. Y.* T. Sterry Hunt.

Boston, July 25, 1873.

4. *On the Situation and Altitude of Mount Whitney;* by W. A. Goodyear, C.E.—On the 27th day of July, 1873, Mr. M. W. Belshaw, of Cerro Gordo, and myself, rode our mules to the highest crest of the peak southwest of Lone Pine, which, for over three years now, has been known by the name of Mount Whitney, and which was ascended and measured as such by Mr. Clarence King, in the summer of 1871. A full account of Mr.

King's ascent of this peak is given in his "Mountaineering in the Sierra Nevada," pp. 264–281.

I know this peak well, and cannot be mistaken as to its identity. As seen from Lone Pine, it appears perhaps the most prominent peak in the whole Sierra; and during the summer of 1870, when, in company with Mr. C. F. Hoffmann and Mr. Alfred Craven, I made a trip for the State Geological Survey through Owen's Valley and the Inyo Mountains, this peak was the object of constant observations by us for a month or more, under the name of Mount Whitney,—which we then supposed it to be. Moreover, since Mr. King's ascent of it in 1871, the half dollar which he left at the summit has been found there, with his name inscribed upon it. There can, therefore, be no mistake as to the identity of this peak with the one ascended and measured by Mr. King in 1871.

I do not mention the fact that Mr. Belshaw and myself reached its summit in the saddle as being one of any new or special interest; for Mr. Sheriff Mulkey, of Inyo County, accomplished the same thing on the 6th day of August, 1872, with his family (i.e., his wife and daughter), and since that time it has also been done by several other parties.

But there is some interest in the fact discovered by Mr. Belshaw and myself, when we reached its summit—that *this peak is not Mount Whitney.*

It is by no means the highest among the grand cluster of peaks which form this culminating portion of the Sierra Nevada; *nor is it the peak which was discovered by Prof. W. H. Brewer and party, in* 1864, *and then originally named by them Mount Whitney.*

For the truth of such a statement as this, after the mountain has become so famous, I shall of course be expected to produce my evidence.

How, then, in the first place, do I know that this so-called Mount Whitney is *not the highest peak* in this vicinity?

First, because on reaching its crest the fact is at once not only apparent, but very striking, even to the unaided eye alone, that a peak which bears N. 67° W. magnetic, distant between five and six miles from the observer, is considerably higher than the one on which he stands. * * *

My second proof of the relative altitude of these two peaks is the following:

I had no spirit-level with me; but I did have a miner's compass, 3¾ inches square, with a clinometer attached. On setting the index of the clinometer at zero, and then sighting along the upper edge of the plate, the line of sight struck far below the summit of the other peak. Then, on reversing the instrument end for end, setting the clinometer again at zero, and sighting along the upper edge of the plate as before, the line of sight, though it struck a little higher than before (thus showing a slight error in the instrument), nevertheless still struck far below the peak. This, if the sighting along the edge of the plate was correctly done, is proof positive that the distant peak is the higher.

But we applied still a third test. While I was busy with my notes, Mr. Belshaw improvised a still more perfect level, by taking a pint cup, four inches in diameter, and filling it *heaping full* of water—i.e., so that the water stood higher than the edge of the cup all around the rim, yet without overflowing. When this was done, it became at once evident, on sighting across the smooth surface of the water, that the other peak was higher than the one on which we stood by an amount which we both of us estimated to be *not less than* 500 *or* 600 *feet.*

As to the proofs that the peak which we climbed *is not the one originally named Mount Whitney by Professor Brewer's party, in* 1864, they are numerous; and among them are the following:

In the first place, Mr. Clarence King, in 1864, on reaching the summit of Mount Tyndall, remarks as follows, in the Geological Survey Report (Geology, vol. I, p. 386):

"On setting the level, it was seen at once that there were two peaks equally high in sight, and two still more elevated—all within a distance of seven miles. Of the two highest, one rose close by, hardly a mile away; it is an inaccessible bunch of needles, and we gave it the name of Mount Williamson. The other, which we called Mount Whitney, appeared equally inaccessible from any point on the north or west side; it is between seven and eight miles distant, in a south-southeast direction, and, I should think, fully 350 feet higher than our peak." (Further investigation showed that it was really 600 or 700 feet higher than Mount Tyndall.)"

Now, the peak which we climbed is certainly not 350 feet higher than Mount Tyndall, but very nearly the same altitude. In fact, as closely as we could judge by our water-level at such a distance, Mount Tyndall appeared a *trifle* the higher of the two. Moreover, this peak, instead of being between seven and eight miles distant in a south-southeast direction from Mount Tyndall, is between twelve and thirteen miles distant from it, in a direction about S. 37° E. true course; while the genuine Mount Whitney (i.e., the highest peak) is actually distant from Mount Tyndall only about seven and one-quarter miles in a direction about S. 26° E. true course—thus corresponding exactly with this remark of King's in 1864. It is evident enough that this difference between seven or eight and twelve or thirteen miles air-line distance involves an error which Mr. King would have been by no means likely to make in his estimate of the distance in 1864; while the direction S. 26° E. also corresponds far more nearly to Mr. King's words, "a south-southeast direction," than the course of S. 37° E. does.

Again, after Mr. King's ascent of Mount Tyndall, and the return of the party to Visalia, Mr. King made another excursion into the mountains, leaving Visalia, July 14, 1864, for the purpose of making an attack on Mount Whitney. He followed from Visalia a trail, which appears, so far as his description and my information give the means of identifying it, to have been the present Hockett Trail, to the point where it crosses the main Kern River. From this point Mr. King followed some route among the upper branches of the Kern River, which he has not described with

sufficient clearness to enable it to be accurately traced on any map with which I am acquainted in the Geological Survey Office, or elsewhere, to the base of Mount Whitney. In his attempt to scale the summit of the mountain, he did not at the time succeed. But the highest point which he reached, as indicated by his barometric observations, was, "according to the most reliable calculations, 14,740 feet above the sea-level." And "at the point where this observation was taken, he was, as near as he was able to estimate, between 300 and 400 feet lower than the culminating point of the mountain, which, must, therefore, somewhat exceed 15,000 feet in height."

Now, although I do not recollect the exact figures which Mr. King's observations in 1871 gave for the height of the peak which he then measured as Mount Whitney, and to whose summit Mr. Belshaw and I rode the other day in the saddle, I do recollect, with certainty, the fact that these figures were a little less than the altitude of the point which he actually reached in 1864, when he was still, according to his own estimate, "between 300 and 400 feet lower than the culminating point" of Mount Whitney.

Here, then, there was a discrepancy of at least 300 or 400 feet, and probably somewhat more, between Mr. King's barometric results in 1864, and his results in 1871; a discrepancy hitherto utterly unaccounted for, and, if the two peaks were identical, unaccountable, except by supposing the existence of errors of a magnitude which is, to say the least, extremely improbable, in the whole method of computing high altitudes from barometric observations. This strange discrepancy vanishes at once when the fact is recognized that, in 1864, Mr. King was attempting a different and a higher peak than the one he climbed in 1871.

Moreover, the shape of the peak and the surrounding country fully justifies me in making the statement that neither Mr. King, nor any other good mountain climber, would ever have reached a point within three or four hundred feet of the summit of the peak which he measured in 1871, and then have given it up in despair. If he had approached this mountain from anywhere on the north or northwest sides, he could never have reached a point so near the summit; for the precipices in these directions are tremendous, for at least a thousand to fifteen hundred feet below the crest; and on the other hand, if he had approached it anywhere from W.S.W., around by south to southeast, he would have gone directly to the summit with no difficulty whatever; for in all these directions the slopes are comparatively smooth and easy.

The following remarks from the Geological Survey Report (Geology, vol. i, pp. 390 and 391), and for which Mr. King's notes of 1864 also furnished the material, will be sufficient additional proof, I think, of the fact, that the peak which for three or four years has borne that name is not the one originally named Mount Whitney:

"Mount Whitney is a ridge having somewhat the outline of a helmet, the perpendicular face being turned toward the east.

There is snow on the summit, which indicates that there must be a flat surface there. The mountain is the culminating point of an immense pile of granite, which is cut almost to the center by numerous steep and almost vertical cañons, ending in high-walled amphitheaters. Southward of the main peak, there is a range of sharp needles, four of which are over 14,000 feet high. The general aspect of the group is much like that of Mount Tyndall. This mountain has been approached on all sides except from the east, and found to be utterly inaccessible. Mr. King thinks it possible, however, that some route may yet he found by which the summit can be reached."

Now, this description corresponds in every respect, so far as Mr. Belshaw and I could see and judge, with the grand peak to the northwest of us—the original Mount Whitney; and it does not correspond at all with the one we were on, and which by mistake has borne the name so long.

Mount Whitney, having "its perpendicular face turned toward the east," looks from Lone Pine like a pretty sharp conical peak. The other peak shows the "helmet outline" from Lone Pine, and its perpendicular face is turned toward the north and northwest instead of the east; while the true Mount Whitney, as seen from the summit of this peak, assumes again the "helmet outline," with the steepest bluff to the eastward.

Again, the peak we climbed is not cut anywhere near to the center by cañons, either numerous or steep, on the south or southwest sides. Furthermore, there is no vestige of a range of "sharp needles" to the south of it, or of anything that could suggest such an idea; while immediately to the south of the towering peak, northwest from the one we climbed, there is precisely such a range of tremendous and utterly inaccessible crags and turrets, and sharp and lofty pinnacles.

The mountain which we climbed also, instead of being inaccessible "on all sides except from the east," is, as already stated, very easily accessible from anywhere from W.S.W., around by south to southeast.

In the face of all these facts, though it may be possible, yet it certainly seems hardly credible, that Mr. King, familiar as he was, or at least ought to have been, long previous to 1871, with the general appearance of the whole region of country immediately to the north and northwest of Mount Whitney, should, on reaching in 1871 the summit of the peak to whose crest Mr. Belshaw and I lately rode our mules, have failed to recognize at once the fact that he was on a lower and different peak from the one he had attempted in 1864. And yet, on the other hand, if he did recognize this fact, then why, on his return from the trip which he made in the summer of 1871, for the special purpose of climbing and measuring Mount Whitney, did he not make it known and give it publicity?

In any case, the fullness of detail with which Mr. King, in "Mountaineering in the Sierra Nevada" (pp. 277 and 278)—

while standing, in reality, on a peak over five miles distant from the one which he says was under his feet—appears to recognize all the topography of the scenes of his earlier struggles, and of his attempts to reach the summit of Mount Whitney, in 1864, is something interesting.

Certain it is, however, that the peak which for over three years has borne the name of Whitney, has done so only by mistake, and that a new name must be found for it; while the name of Whitney must now go back to the peak to which it was originally given in 1864, and which, is, in reality, the highest and grandest of this culminating cluster of the Sierra Nevada.

Furthermore, it appears that Mount Whitney not only retains its claim to being the highest point of land in the United States of America, but that its claim to over 15,000 feet of absolute altitude above the sea is still indisputable; while, up to the present time, it also retains the prestige of the fact that, in all probability, no human foot has ever trodden its summit.

If Mr. King's descriptions, in 1864, of the appearance and surroundings of this mountain on the north and northwest can be relied upon, it is safe to say that no man will ever ride a horse or mule to the summit of *that* peak, unless it be by a costly as well as a dangerous trail.

Whether the peak is utterly inaccessible or not, is still a question. I am disposed to think that it can be climbed; but it will certainly involve a great deal of hard and, very possibly, some dangerous work for anybody who shall attempt to reach its gigantic crest.

NOTE.—Aug. 6: I have just received from Mr. Belshaw the results of a rough triangulation made by him from Cerro Gordo to the summits of the two peaks in question, since my return.

The figures given by this triangulation, though not to be relied upon as very accurate, are still sufficiently so to show clearly the relative situation of things, and to furnish additional confirmation of the facts as stated in the above paper.

He makes the air-line distance from Cerro Gordo to the peak measured by Mr. King, in 1871, in a course S. 72° W. magnetic, 25 miles, and the altitude of this peak 14,033 feet. The distance to the genuine Mount Whitney he makes 30·18 miles, in a direction S. 80° 5′ W. magnetic, and its altitude 14,930 feet.

Both these altitudes are probably too low; but there can be no question as to which is the higher peak.—*Proc. Cal. Acad. Sci.*, Aug. 4, 1873.

5. *Sixth Annual Report of the U. S. Geological Survey of the Territories, embracing portions of Montana, Idaho, Wyoming and Utah; being a Report of Progress of the Explorations for the year* 1872; by F. V. HAYDEN, U. S. Geologist. 844 pp. 8vo. Washington, 1873. Conducted under the authority of the Secretary of the Interior.—Dr. Hayden's Report for 1872 comes laden with valuable information from the mountains. His explorations are rapidly extending our knowledge of the topography and resources of the mountain territories, and not less enlarging the bounds of science. The region of the headwaters of the Yellowstone has now excellent maps, lakes their right positions and connections, and a park, rendered attractive by the most wonderful fountains the world contains, has been opened to

the public. The volume commences with the special report of Dr. Hayden, containing new facts, both economical and scientific, from the region of the geysers, and the country surrounding, with a reconnoissance of the mining region on Clark's Fork of the Yellowstone, and a partial résumé of previous investigations. New sketches of scenery are contained in the volume, with several new maps, and many geological sections. Dr. Hayden states in his introduction that a large series of maps will soon be got out by Mr. G. R. Bechler, the topographical surveyor of the expedition, only some of the smaller of which appear in this volume.

The Report of Dr. Hayden is followed by those of other members of the expedition.

A. C. Peale describes the geological and mineralogical character of the region visited, giving details with regard to the features and mineral products, and the hot spring regions, the composition and temperature of the waters, composition of the siliceous depositions, the character of the eruptions and their craters, together with various geological facts of value, and a list of the minerals and rocks obtained in the course of the survey.

The Report of Frank H. Bradley, geologist of the Snake River division, announces, among its many interesting facts, the discovery of the Quebec group (the age between that of the Calciferous sandrock and Chazy limestone) in Wyoming Territory —rocks hitherto not found out of Canada excepting by Prof. Bradley near Knoxville, East Tennessee. An abstract of the report of Prof. Bradley, by himself, has been already given on pages 194 to 206 of this volume.

The physical geography and agricultural resources of Minnesota, Dakota and Nebraska, are well described in the following report by Cyrus Thomas.

Part II. is occupied with special Reports on Geology and Paleontology.

Mr. L. Lesquereux has an admirable Report on the Coal or Lignitic formation of the Rocky Mountain territories, and its flora —as already announced in this volume. The subject is discussed with great thoroughness, and over a hundred new species of fossils are described.

H. M. Bannister gives a report of a geological reconnoissance along the Union Pacific Railroad.

Then follows the Paleontological Reports of F. B. Meek, describing the Paleozoic and later formations, and their invertebrate fossils; of E. D. Cope, on the extinct vertebrates (mammals, reptiles, and fishes) of the Wyoming Eocene, with notes on the geology, illustrated by two plates of the *Synoplotherium lanius* Cope, and four of the *Loxolophodon cornutus* Cope; also a paper on primitive art in the Bridger Basin, by J. Leidy, and another on the ancient mounds of Dakota, by C. Thomas.

Part III. consists of special reports on the zoology and botany of the territories: on the mammals and birds of the expedition, by C. H. Merriam; the Coleoptera, by G. H. Horn; Orthoptera, by

C. Thomas; the Odonata, by H. Hagen; New Mallophaga, new parasitic worms found in the brain and other parts of bodies of birds, new insects, and on the insects of the Great Salt Lake, by A. S. Packard, Jr.; Botany, by J. M. Coulter; Cyperaceæ, by S. T. Olney; Graminaceæ, by G. Vasey; Musci, by L. Lesquereux; Lichens, by H. Willey; Fungi, by C. H. Peck.

Part IV. contains the report on astronomy and hypsometry of H. Gannett, giving a great number of heights along different routes over the mountains, besides other information; notes on the climate of Montana, by Granville Stuart, and a résumé of meteorological observations at Fort Ellis, Montana.

Besides working himself, as has been seen, Dr. Hayden has had the coöperation of some of the ablest scientific men of the country. We propose to cite from the volume at another time.

Dr. Hayden has recently been elected a corresponding member of the Academy of Sciences at Liége.

6. *The Geological evidences of the Antiquity of Man, with an outline of Glacial and Post-tertiary Geology, and remarks on the origin of species with special reference to Man's first appearance on the earth;* by Sir CHARLES LYELL, 4th edition, 572 pp. 8vo. 1873. London and Philadelphia (John Murray, London; J. B. Lippincott & Co., Philadelphia).—This 4th edition of Lyell's well-known work on the Antiquity of Man brings the subject down to the present year. The author's wide range of observation, and his extreme care in sifting facts, and gathering in all that is pertinent, make his works a reliable and to a large extent an original source of information for all who wish knowledge on the subject, whether they agree with him in some of his conclusions or not. The volume is printed in the best style, and has a number of good illustrations.

7. *Geological Survey of Wisconsin.*—The government of Wisconsin has authorized a geological survey of the State, and it has been going forward through this summer. The geological corps under appointment consists of—Chief, I. A. LAPHAM, LL.D., Milwaukee; Asst's, ROLAND D. IRVING, A.M., E.M., Madison, T. C. CHAMBERLIN, A.M., Whitewater, MOSES STRONG, A.M., Mineral Point; Chemist, W. W. DANIELLS, M.S., Madison.

8. *Pareira Brava*, the root of which, once famous in medicine, is still important, has long been generally supposed to be *Cissampelos Pareira*. In the Pharmaceutical Journal, August, 1873, Mr. Hanbury convincingly makes it clear that the source of the root is *Chondodendron* (or more correctly *Chondrodendron*) *tomentosum* of Ruiz and Pavon. With this accords the Portuguese name, *Parreira*, signifying "a vine that grows against a wall or over an arbor," and *Pareira Brava* is equivalent to Wild Vine. The foliage of the true plant is not unlike that of the vine, and the fruit resembles a bunch of grapes. A wrong and perfectly inert root has of late years come into the market—not however a *Cissampelos;* but the true root now being known, it would advantageously replace the worthless kind found in the drug trade.

A. G.

9. Elias Durand died at his residence in Philadelphia, on the 14th of August, in this year, so fatal to American botanists. He was one of the oldest, having been born at Mayence, France, on the 25th of January, 1794, and was therefore in the 80th year of his age. He used to tell how he was baptized into botany upon the field of the battle of Leipsic: while serving as *aide-Pharmacien*, the medicine wagon under his charge was brought to a halt almost under fire on the edge of a bog, where he first saw and collected *Menyanthes trifoliata*, and so began his herbarium and the botanical study, which he kept up with order and delight until the failure, first of eye-sight and then of his mental powers, a year or so ago. He had amassed a large and very valuable North American herbarium, which, about five years ago (after consulting the writer of this notice), he presented to the museum of the *Jardin des Plantes* at Paris, desirous, like a true son of France, to do something by which his favorite science might be advanced, and his name honorably remembered, in his native land. No sooner was this, the work of a long life-time, safely deposited in its appropriate final resting place, than Mr. Durand began to devote the remainder of his energies to the formation of a supplementary herbarium, which, when he could do no more, was lately sent on to Paris.

Mr. Durand's principal botanical writings are his *Plantæ Hermannianæ*, and *Plantæ Prattenianæ*, accounts of interesting Californian collections which were published first in the Journal of the Academy of Natural Sciences, and then, more fully and with plates, in the Pacific Railroad Exploration Reports; also a sketch of the Botany of the Basin of the Great Salt Lake of Utah, and enumeration of the plants collected by Dr. E. K. Kane in his first and second expeditions to the Polar regions, also contributed to the Journal of the Academy of Natural Sciences.

Mr. Durand came to the United States in the year 1806, was for some years chief clerk and afterward a partner in Mr. Ducotel's druggist establishment in Baltimore, but in 1825 he established himself as an apothecary and druggist in Philadelphia, where his store, at the corner of Sixth and Chestnut streets, soon became renowned, and its proprietor took rank at the head of his profession. He pursued it as a learned profession, secured the unlimited confidence and regard of the distinguished medical men of the city, as well as of the public; and, as age drew on, retired with a handsome competency to the more exclusive pursuit of his favorite science. He has been an active correspondent, friend, and helper of all the principal botanists of the country, from Zaccheus Collins and Nuttall down to those who have but lately entered upon the field. With Torrey, whom he has so soon followed, and with his associate, the writer of this notice, his relations were continual and intimate. The frequent mention of his name on the pages of their works bears honorable but very inadequate testimony to his botanical services and usefulness. All those who, like the survivor, were honored by his friendship, will hold in affectionate

remembrance the charming personal qualities, the unselfish zeal, the genial kindness, and the true benevolence of the good old man who has now gone to his rest. A. G.

August 18th.

III. Miscellaneous Scientific Intelligence.

1. *Meeting of the American Association for the Advancement of Science, at Portland, Maine, commencing Wednesday, August* 20, 1873.—The meeting of the Association at Portland was very largely attended, and, with few exceptions, excellent in its scientific papers. Professor Joseph Lovering of Cambridge was President. The members had a generous reception from the citizens of Portland, and enjoyed much the excursions which had been projected for the week. The Association adjourned on Tuesday, August 26, to meet at Hartford, Conn., on the second Wednesday of August, 1874. The officers elected for the next meeting are Dr. J. L. LeConte of Philadelphia, Prof. C. S. Lyman of New Haven, Dr. A. C. Hamlin of Bangor, General Secretary, and Mr. F. W. Putnam of Salem, Mass., Permanent Secretary.

The following are the titles of the papers accepted for reading at the sessions.

On the Possibility of a Liquid Solar Envelope; C. A. Young.

Rotation of the Planets as a Result of the Nebular Theory; B. Peirce.

Relation of Internal Fluidity to the Precession of the Equinoxes; J. G. Barnard.

On the Equilibrium and Dynamic Theories of the Tides; J. G. Barnard.

Meridianal Arcs Measured in the Progress of the Coast Survey; J. E. Hilgard.

On the Longitudes of Greenwich, Paris, and Harvard Observatories, determined by the U. S. Coast Survey; J. E. Hilgard.

On Solar Disturbances of the Magnetic Needle; J. E. Hilgard.

On Methods of Determining the Ratio of Volume and Weight of Water; id.

A New Curve; T. Hill.

Three Equations Partially Discussed; T. Hill.

An Attachment to the Whirling Table for projecting Lissejous' Curves; A. E. Dolbear.

On the Convertibility of Sound into Electricity; A. E. Dolbear.

A New Method of Measuring the Velocity of Light; A. E. Dolbear.

On the Periodic Error of the Right Ascensions of the Nautical Almanac, and its Effect on the Longitudes which depend on them; W. A. Rogers.

The Coefficient of Safety in Navigation. An attempt to ascertain within what limits a ship can be located at sea by astronomical observations; W. A. Rogers.

Musical Flow of Water; H. F. Walling.

Dissipation of Energy; H. F. Walling.

Remarks on a projected Gigantic Telescope; P. H. Van der Weyde.

Microscope with a new style of Micrometer, and Remarks on the Method of enlarging the Field; id.

Investigation into the True Cause of a peculiar form of Mirage; id.

On the Telescopic Study of the Solar Photosphere; S. P. Langley.

Angular Aperture of Immersion Objectives for the Microscope; R. H. Ward.

The Cohesion of Liquids; G. J. Wardell.

The Telescope and the means of improving it, and also the Utilization of Solar Heat; G. W. Holley.

Dissociation of Water by Heat as a cause of steam-boiler explosions; L. Bradley.

The Chemistry of Copper Matte; T. S. Hunt.

On the Silt Analysis of Soils and Clays; E. W. Hilgard.

Analysis of Mississippi Soils and Sub-soils; E. W. Hilgard.

On the Distribution of Soil Ingredients in the Sediments obtained by silt analysis; R. H. Loughridge.

On the Influence of Strength of Acid and Time of Action on the Results of Chemical Soil Analysis; R. H. Loughridge.

The Atmospheric Electricity of the Earth, of the Sun, and of the Comets; and the Physical Constitution of the Sun and of the Comets; J. Ennis.

New Theory of Geyser Action; E. Andrews.

Cold Water Condensers; J. B. Walker.

On Heating Iron by Hammering; F. W. Clark.

Plate Lime-glass, and the Manufacture of Glass; L. Feuchtwanger.

An Automatic Filtering Apparatus. Presentation of the Apparatus at work, described in May number of Am. Jour. Sci.; H. W. Wiley.

Automatic Registering and Printing Thermometer; G. W. Hough.

Automatic Registering and Printing Evaporator and Rain Guage; id.

Tornadoes of Illinois; M. L. Comstock.

On the Storms of Western Illinois; M. L. Comstock.

Peculiar Formations of Wind in Local Thunder Storms; H. A. Cutting.

The Arctic Regions; W. W. Wheildon.

On the Unification of Doses and the Introduction of the Metric System into Medicine; H. W. Wiley.

Method of Harmonizing Apothecaries' and the Metric System of Weights; E. B. Elliott.

Metric and Radial System of Measures of Length; E. B. Elliott.

2—*Statistics.*

Unreliability of Life Statistics as usually compiled; T. S. Lambert.

On the Credit of the U. S. Government, as indicated by the daily market quotations of prices of its Securities; E. B. Elliott.

Periodicity of Rates of Interest in the New York Market; E. B. Elliott.

International Coinage—its progress; E. B. Elliott.

Irregularities in the returns of the population of the U. S. Census of 1870, at earlier ages, with methods and results of correction and adjustment; E. B. Elliott.

Life Table, Table of Mean Future Duration of Life, and Table of Life Annuity, on the Basis of the U. S. Census of 1870, with Method of Construction; id.

3—*Geology, Natural History, etc.*

Lateral Position of the Vent in Amphioxus, and in certain Batrachian Larvæ; B. G. Wilder.—Present aspect of the question of Intermembral Homologies; id. Variation in the condition of the Sense Organs in Fœtal Pigs of the same litter; id.—Lateral Asymmetry in the Brains of a Double Human Monster; id.—The Papillary Representation of two Arms in a Double Human Monster; id.—Variations in the Cerebral Fissures of Domestic Dogs, and its bearing upon Scientific Phrenology; id.—The Pectoral Muscles of Mammalia; id.—Variation in the Pectoral Muscles of Domestic Dogs; id.—The Cerebral Fissures of Mammalia and the limits of their homologies; id.—The Habits and Parasites of Epeira riparia; id.—The nets of Epeira, Nephila, and Methros; id.—Composition of the Carpus in Dogs; id.—The need of a Uniform Position for Anatomical Figures; with a recommendation that the Head be always turned toward the left; id.

Some remarks on the Zoological and Physical Explorations of the New England Coast by the U. S. Fish Commission; A. E. Verrill.

Animal Organology; T. C. Hilgard.

Relations of Dentalium; E. S. Morse.

Genitalia of the Brachiopoda; E. S. Morse.

Embryology of the Brachiopoda; E. S. Morse.

Further Remarks on the Embryology and Morphology of Limulus; A. S. Packard, jr.

Note on Bufo Americanus; T. Hill.

On the Genus Mycropterus (Grystes) and its Species; T. Gill.

Hints for the Promotion of Economic Entomology in the U. S.; J. L. LeConte.

On the Origin of Species; G. C. Swallow.

On means of distinguishing between Vegetable and Animal Life; T. S. Lambert.

Remarks on the Origin of Insects and on the Antennal Characters in the Butterflies and Moths; A. R. Grote.

Notes on Liparis, Cyclopterus and their allies; F. W. Putnam.

On the Question, "Do Snakes Swallow their Young?" G. B. Goode.

Concerning Hyalonema; S. Lockwood.
The Museum of Natural History in Central Park, New York; A. S. Bickmore.
Suggestions for Facilitation of Museum Administration; T. Gill.
The Identity of the Locust of the Prophet Joel (ii, 55) with the Œdipoda migratoria of Europe; C. Thomas.
On recent additions to the Fish Fauna of Massachusetts; T. Gill.
On Hermaphroditism in Rhus cotinus and R. glabra; T. Meehan.
Some Botanical Contrasts of Portland with New York city; J. Hyatt.
On the effects of certain poisons on Mollusks; W. N. Rice.
On a Remarkable Group of Wasp's Nests found in a hollow stump; P. R. Uhler.
On Movement in the Stigmatic Lobes of Catalpa; T. Meehan.
Calvert's supposed relics of man in the Miocene of the Dardanelles; G. Washburn.
On the Marble Deposits of Pottsford, Vermont; J. S. Newberry.
On the Age and Structure of the Cincinnati Anticlinal; J. S. Newberry.
On Circles of Deposition in American Sedimentary Strata; J. S. Newberry.
On some Paleozoic Fishes from the rocks of Ohio; J. S. Newberry.
On the Geological Relations of the Niagara and Lower Helderburg Group of Rocks in the United States and Canada; James Hall.
Breaks in American Paleozoic Rocks; T. S. Hunt.
The Geology of Southern New Brunswick; T. S. Hunt.
The Metamorphoses of Rocks; T. S. Hunt.
The Geology and Economic Mineralogy of the Southern Appalachians; id.
On Staurolite Crystals and Green Mountain Gneisses of Silurian age; J. D. Dana.
The Slates of the Taconic Mountains of the age of the Hudson River or Cincinnati Group; J. D. Dana.
On some extinct species of horned perissodactyls; E. D. Cope.
The Devonian Limestone in Ohio; N. H. Winchell.
Geology of the Northwest part of Maine; C. H. Hitchcock and J. H. Huntington.
Note on the Cretaceous Strata of Long Island; C. H. Hitchcock.
Geological History of Lake Winnipiseogee; C. H. Hitchcock.
Geology of Portland and vicinity; C. H. Hitchcock.
Means of determining the Stratigraphical Order of Seams of Coal in Ohio, Kentucky, etc.; E. B. Andrews.
Geological Relations of the Iron Ore Deposits of Nova Scotia; J. W. Dawson.
Specimen of Sigillaria showing marks of Fructification; J. W. Dawson.
Zones of Parallel Lines of Elevation in the Earth's Crust; A. Ross.
The Quartzite of Williamstown and vicinity and the Structure of the Graylock Range; S. Tenney.
Exhibition of Marl Fossils from New Jersey, near the coast; L. Feuchtwanger.
Artificial Shell Heaps of Fresh-water Mollusks; C. A. White.
The largest Tooth of the Fossil Elephant yet described; E. O. Hovey.
On some Spontaneous Fractures, Movements, and Expansions of Rock observed in Munson, Mass.; W. H. Miles.
On an Ancient Burial Ground in Swanton, Vermont; G. H. Perkins.
Exhibition of Colored Diamonds and other gems, with brief remarks on their formations; A. C. Hamlin.
On the Origin of Mountain Chains; C. Whittlesey.
On the cause of the Transient Fluctuation of Level on Lake Superior; id.
On the Descent of the Rivers of the Valley of the Missisippi; C. Whittlesey.
Natural Features of the U. S. National Park in the Rocky Mountains; J. Curtis.
Architecture of the American Aborigines; L. H. Morgan.

2. *Popular Lectures on Scientific Subjects;* by H. Helmholtz, with an Introduction by Prof. Tyndall. 388 pp. 12mo. New York, 1873 (D. Appleton & Co.). The lectures of Helmholtz, the leading physicist of Germany, here presented in an English form through translations by different authors (among them Tyndall), are on—The relation of Natural Science to science in general; Goethe's scientific researches; the physiological causes of harmony in music; ice and glaciers; the interaction of the natural forces;

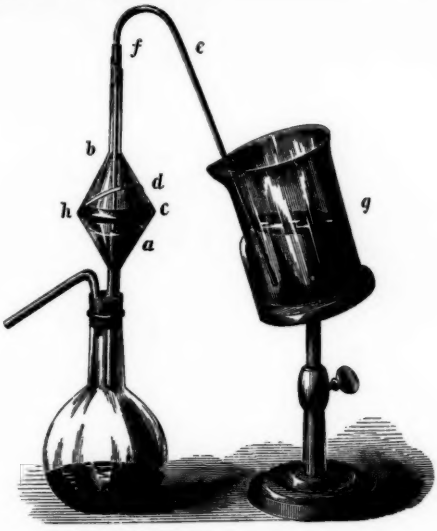
the recent progress of the theory of vision; the conservation of force; and the aim and progress of physical science. They are all of the widest general interest, and are treated in a simple and popular manner. The volume has several wood-cut illustrations.

3. *Minerals Springs of North America, how to reach and how to use them;* by J. J. MOORMAN, M.D., Physician to the White Sulphur Springs, Virginia. 394 pp. 12mo. 1873 (Lippincott & Co.). A popular work, containing notices of many of the mineral springs of the continent, intended for the tourist and invalid. 182 pages out of the whole are devoted to Virginia Springs. Some analyses are given, and also the "thermalization"– in ordinary and much more correct language, temperatures—of various warm and hot springs of the United States. We leave it for medical men to pronounce on its medical opinions.

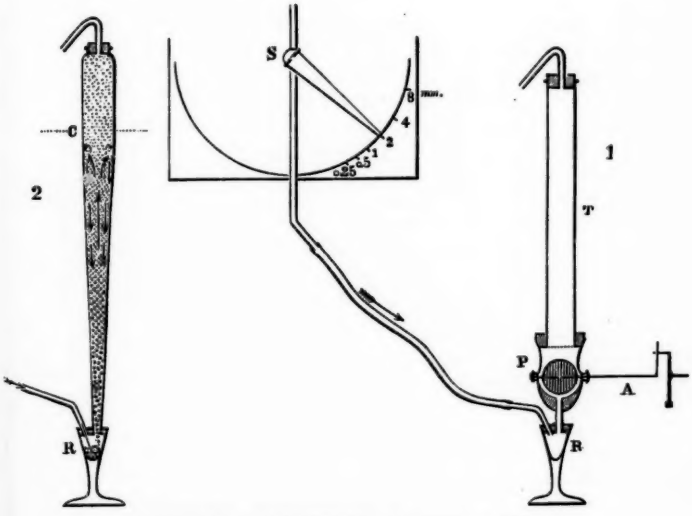
4. *Foods;* by EDWARD SMITH, M.D. 485 pp. 12mo. New York, 1873. (D. Appleton and Co.)—This work is one of Appleton's "Scientific Series," of which Tyndall's Form of Water and Bagehot's Physics and Politics have already been noticed.

Dr. Smith is widely known as a physiologist who has in his own person carried out a large number of experiments on the influence of food and drinks upon respiration, nutrition and digestion. He handles his subject, therefore, with the power of an original investigator. His book is replete with facts and experimental results, and will be read with interest by a large class of non-scientific readers as well as specialists. The American reader must be pardoned for smiling at some of his statements respecting familiar facts in this country: as for example, that the "juices" of the sugar maple "are very abundant in spring and summer," that the tree "resembles the sycamore tree," and that the "other flavors besides those due to sugar," he thinks, would "occasion it not to be preferred to the purified cane sugar where that could be obtained at about the same cost;" while this is the very reason why the maple sugar commands in our cities double the price of refined cane sugar. But we will leave our usually accurate author to settle this little difference with "Young America," who will be as a unit against his statement. Not less will the American whisky drinker be surprised to hear that the "peach whisky" is named first of those kinds "accounted the best." His chapter on "Alcohols," which follows that on Tea, Coffee, etc., as Liquid Foods, is the one of all others on which opinion will be most sharply divided, as he frankly says, both on "the question of their right to be called foods, and upon moral grounds, they are driving civilized nations into two hostile camps." But however men may differ, the experimental results recited by Dr. Smith on his own person and others, bring the subject within the range of scientific research.

Dr. Smith's statements as to the nearly valueless character of the Liebig beef extract as nutriment, have excited much discussion, but he sustains them by arguments founded on analyses and observation. The book is full of valuable hints for the physician. It is to be followed by another on Diets.



FILTERING APPARATUS, Art. XXV, p. 214.



HYDRAULIC ELUTRIATORS, Art. XXXI, p. 288.